Empiricism in the Philosophy of Science

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Abstract

There are two main aims of this thesis: the first is to demonstrate that there is an important version of empiricism — “methodological empiricism” — which is a central part of the empiricist tradition but has been neglected in current philosophy of science. The second aim is to develop methodological empiricism in light of current science. The first aim is met by first articulating what I take methodological empricism to mean, alongside articulating the more dominant version of empiricism — “epistemic empiricism”. I explicate both via several characteristics for each, and then trace a history of both positions from Ancient Western philosophy up until current times. Finally, I give evidence of the neglect of methodological empiricism in current philosophy of science. The second aim is met by, first, presenting four criteria for a current version of methodological empiricism that are directly derived from the characteristics of methodological empiricism through its history. I then consider three topics within recent philosophy of science that *prima facie* pose a challenge to methodological empiricism, all of which can be broadly characterised as appearing to be non-empirical in some way — analogue confirmation, philosophy of computer simulations, and non-empirical theory confirmation. It is argued that, ultimately, analogue confirmation and computer simulation are compatible with methodological empiricism, but that non-empirical theory confirmation is not. I argue that this should gives us good reason to reject non-empirical theory confirmation.

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Author's Declaration

I declare that the work in this dissertation was carried out in accordance with the requirements of the University's Regulations and Code of Practice for Research Degree Programmes and that it has not been submitted for any other academic award. Except where indicated by specific reference in the text, the work is the candidate's own work. Work done in collaboration with, or with the assistance of, others, is indicated as such. Any views expressed in the dissertation are those of the author.

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Contents

Introduction

Everyone with some sort of education in philosophy has at least some understanding of empiricism. It is taught in introductions to general philosophy, and in philosophy of science most students of philosophy will likely become aware of constructive empiricism and the logical positivists when learning the basics of the sub-discipline. The logical positivists — staunch empiricists — are surely the most iconic philosophical movement within the philosophy of science, and van Fraassen — in some ways the heir to the logical positivist's empiricist project — is certainly one of the most well-respected and most discussed philosophers in current philosophy of science. Books are routinely published on the topic of empiricism, it has entries in both Wikipedia and the Stanford Encyclopedia of Philosophy, and even has an episode dedicated to explicating it on BBC's "In Our Time". Thus, a question that strikes one may be: why do we need more research into empiricism?

A central claim of this thesis is that current philosophy of science operates with a very narrow view of what empiricism *is*, what it *has been*, and what it *could be*. This overly narrow understanding of empiricism that exists now — the version of empiricism that most will point to as "empiricism" — has certainly been heavily researched. But this is not all that empiricism is, nor what it has been, nor what it could be. Nor is it, I will go on to argue, what empiricism *should* be.

This project has two main aims. The first is to demonstrate that there exists an alternative form of empiricism to this aforementioned orthodox conception of empiricism in current philosophy of science, and that this alternative version has been heavily neglected in current philosophy of science. The orthodox version of empiricism I denote, borrowing Allen's (2021) term with slight modification, "epistemic empiricism" (EE); the other version I term "methodological empiricism" (ME). ME has been neglected in recent times. This demonstrates that EE — despite undeniably being the dominant conception of what empiricism is in current philosophy of science — is not all that empiricism is, nor is EE all that empiricism has been, nor is it all that empiricism could or should be.

In current philosophy of science EE is simply called "empiricism" and is usually taken by philosophers to explicitly involve an anti-realism of some variety, demarcating observables and unobservables around sensory perception and prioritising the former. It is also taken to reject metaphysics. More implicitly, I claim, it includes an individualistic stance that fits into the Cartesian tradition, a focus on theory over practice, and some sort of epistemic scepticism. ME, differing from EE, focuses centrally *not* on epistemology, but on empirical investigation into the world. There is no particular stress on the importance of sensory perception, or on what is observable-for-humans, there is often a metaphysics provided that is naturalistic, and those in the ME tradition are typically scientific realists. They tend to be focused on practice rather than on theory, and the scepticism that they do embrace is far less radical than those in the EE tradition.

Both EE and ME have long and rich histories. The former is typically associated with Locke, Berkeley, and Hume. These are labelled the "Early Modern", or "British" empiricists. They all focused — the story goes — on rejecting innate ideas and innate knowledge, and had their philosophical enemies in the form of the continentally based *rationalists*, who are typically listed as Descartes, Spinoza, and Leibniz. This is a simplified version of the orthodox conception of empiricism, and is established as the dominant narrative in Britain somewhere between 1895-1915 (Vanzo, 2016).[1] The history of ME is not really known. This is unsurprising, given that philosophers in current philosophy of science do not seem to recognise its current existence. It is vital, though, to set out this history of ME, in order to show the importance of it as a position and to show that it is not something arbitrary that can be dismissed. ME begins properly with Francis Bacon, although it has precursors in the Medieval period including Grosseteste and Roger Bacon. Other important figures broadly fit into it. A non-exhaustive list includes Boyle, Hooke, Whewell, Herschel, Dewey, James, Neurath, and Cartwright.

The second aim of this project builds directly from the first aim. Whereas the first aim seeks to establish the existence of ME and to demonstrate its neglect, the second aim explores what ME would properly look like when applied to current science. A normative dimension exists in this, also: it is claimed that this way of formulating ME is the best way to formulate

[1]This is discussed in more depth in (1.1).

empiricism, and that if one is going to be an empiricist in philosophy of science, this is the position that one should hold. This therefore explores the potential of what ME could be, if formulated properly.

The first aim is achieved in the first three chapters, which form part I of this project: *what empiricism is, and what empiricism has been.* Chapter 1 sets out what empiricism is, in broad enough terms that are sufficient to capture all variations of empiricism, past and present. I define both EE and ME via a set of criteria, with a brief explication of each of these criteria. The remainder of chapter 1 and then all of chapter 2 sets out the history of these two versions of empiricism, including proto-forms of both which exist throughout the history of western philosophy. Some time is spent on the Ancient Greeks and Medieval philosophers, explicating how certain parts of specific philosophers' views are important precursors to empiricist positions, and then a history of empiricism proper is traced from the empiricism of Francis Bacon up until the empiricism of Hans Reichenbach and Carl Hempel. This history is given to highlight that it is simply a matter of fact that there exists a plethora of thinkers in the history of empiricism who have not conformed to EE, and in fact fit better into the version of ME. Chapters 1 and 2 are thus concerned with (i) setting out what these versions are, and (ii) establishing the existence of these two versions of empiricism, historically, with the clear message that EE is not an exhaustive account of empiricism from the perspective of the history of empiricism. Empiricisms from other traditions other than Western philosophy are not considered.[2]

Chapter 3 is concerned with establishing the existence of EE and ME in current philosophy of science, and so I turn to current manifestations of the two versions of empiricism. The two philosophers that are the most prominent manifestations of these versions of empiricism are Nancy Cartwright (ME) and Bas van Fraassen (EE), and are consequently focused on. In depth explications of both philosophers are given, and I make clear exactly how the former's empiricism fits into EE, whilst the latter's fits into ME. This chapter concludes the first aim and ties this first part of the project together. I spell out how current philosophy of science seems to consider empircism as exclusively being restricted to EE, with some explicit examples of how this is the case.

The second aim is achieved in chapters 4 and 5, which comprise part II of this project: *what empiricism should be.* As the name of part II suggests, this is is much more prescriptive than part one. In chapter 4 I use the six

[2]For histories of empiricism that include non-Western empiricisms, see (Allen, 2021).

characteristics of ME that were presented and explored in part I to develop a position of ME that is best suited to current science. This is done by presenting four criteria that are directly derived from said characteristics of ME. The first is that it must allow for the collective nature of science. The second is that it should be able to embrace the shift that has occurred in philosophy of science towards an increase focus on scientific practice as opposed to predominantly focusing on theories. The third is that empiricism should not give epistemic privilege to what is observable by the naked eye, but should instead epistemically privilege "measurables". The fourth is that it should hold some form of causal realism. There is a sort of naturalist presupposition here: that empiricism that fits into the philosophy of science ought to be able to tie itself into current science. This is not argued for — I take it for granted that philosophy of science should be compatible with science.

In chapter 5 I address ME in light of three research topics in current philosophy of science that *prima facie* pose a problem for ME — and empiricism more generally — because of their apparent non-empirical focus. These are analogue confirmation, philosophy of computer simulation, and non-empirical theory confirmation (NETC). The first two study the developments in science of analogue experiments and computer simulations, respectively. These fields are motivated by attempts to study phenomena that are inaccessible via more traditional scientific procedures such as physical experiments where one actually interacts with the system in question. The latter, NETC, is motivated by string theory and its inability to be confirmed by more traditional empirical methods due to not making testable, empirical predictions. I give an explication of each field in turn, and then subsequently claim that the philosophy of computer simulations and analogue confirmation are not in conflict with ME, but NETC is. I argue that since string theory is a highly contested research programme, then we should side with ME and reject NETC.

Part I

What Empiricism Is, and Has Been

Chapter 1

Defining Empiricism, and the History of Empiricism from the Pre-Socratics to Hume

Introduction

A central claim of this project is that there are two versions of empiricism, epistemic empiricism (EE) and methodological empiricism (ME), and that the latter has been neglected in current philosophy of science. This neglect typically comes in the form of philosophers of science unknowingly conflating empiricism — a broad and complex position — to just EE. This is not done intentionally, but because EE has simply become synonymous with empiricism within the philosophy of science.[1] In this chapter I both explicate in depth what I take EE and ME to be, and set out part one of two of a history of both of these empiricisms, with particular reference to the philosophy of science.

First, I define both EE and ME. EE is something like the "textbook" account of empiricism, whilst ME provides an empiricism that is less familiar but — as will be shown — is integral to empiricist thought, historically. Both EE and ME focus on "experience". Consequently, before explicating each position I begin with a discussion of what experience is and how experience is used. I then go on to define both EE and ME via a set of criteria.

[1]Why this is the case will be explored in (3.4)

This comprises the first half of this chapter. The second half works towards establishing this aforementioned central claim of the project by providing a history of empiricism that makes clear that ME is just as historically important as EE. Thus, whilst many philosophers may not initially recognise ME as empiricism as they know it, the purpose of the historical account is to show that ME has played a hugely important part in the history of empiricism, and has always existed alongside EE.

This chapter is the first of two that gives this history of empiricism, and the historical focus in this chapter is on empiricism before Kant. Although nothing like empiricism as we know it now properly existed, I claim, until Francis Bacon, I begin the history in pre-Socratic times. The pre-history of empiricism is included because it gives important insights into empiricism. The Hippocratic physicians, Aristotle, the Pyrrhonists, Epicurus, the Empirics, and the Stoics are examined in the ancient tradition; Robert Grosseteste, Roger Bacon, and the nominalists are examined in the Medieval tradition. The Early Modern period is where we properly see the birth of both EE and ME. Accounts of the empiricism of Gassendi, Locke, Berkeley, and Hume are given and shown to be in the former version. Accounts of the empiricism of Francis Bacon, Boyle, and Hooke are given and shown to be in the latter version.

In discussing the history of empiricism in this chapter, I have highlighted in bold the characteristics that each philosopher satisfies from either EE or ME, in order to make it much clearer to the reader how each philosopher's empiricism fits into either EE or ME.

1.1 Two versions of Empiricism

The orthodox picture of empiricism that is typically taught or held is something like this:

Empiricism is a version of thought that began in Early Modern Britain and was dominated by three thinkers: Locke, Berkeley, and Hume. Locke began the tradition, then Berkeley developed on and corrected some points of Locke and made the position more extreme. Finally, this empiricist programme culminates in Hume who takes it to its logical conclusion by building on and correcting elements of both Locke and Berkeley. The empiricists were fundamentally opposed to the "rationalists", with the disagreement between the two boiling down to an epistemic disagreement about the nature of (i) innate ideas and (ii) whether or not we can have a priori knowledge of the world. Kant then went on to synthesise the two positions.

This will be referred to as the "orthodox narrative". According to Vanzo (2016), this is established as the dominant narrative somewhere between 1895-1915 through several different authors, each adding particular details and additional components. These authors include Thomas Reid, Kant and his disciples — including Tennemann and Fischer — and Hegelians in the 19th century, especially Thomas Hill Green (Vanzo, 2016). This narrative has been criticised by historians of philosophy since around the 1980s (Norton, 1981)(Loeb, 1981) for its over simplification and historical inaccuracies. Norton (1981) criticises the narrative for suggesting that the tradition begins with Locke and is confined to Britain (Norton, 1981, p334), suggesting the French Pierre Gassendi as the founder of modern empiricism instead. According to Norton, Berkeley and Hume's empiricism are not just expansions of Locke's empiricism, and both owe large debts to "rationalist" philosophers. Vanzo (2016) points out that Kant viewed himself as a particular kind of rationalist (ibid, p260) and never saw himself as a synthesizer.[2]

With this being said, and accepting that the orthodox narrative is overly simplistic and restrictive, it is certainly true that Locke, Berkeley and Hume were important empiricists in whatever way we want to now understand empiricism. But a central claim in both this thesis and chapter is that they are emblematic of only one version of empiricism. I take Locke, Berkeley, and Hume — amongst others — to be paradigmatic of the tradition of "epis-

[2]Reinhold is the first to incorporate the "Kant as synthesizer" element into this narrative, and does so in the late 1700s

temic empiricism".[3] That is the standard conception of "empiricism". Before laying out EE, some important points should be clarified.

The orthodox narrative of empiricism has two central features. The first is a rejection of innate ideas; the second is, positively formulated, that all knowledge originally derives from experience. The second, in negative formulation, is that there is no a priori knowledge of the world. I do not include either of these features of the orthodox narrative in the characterisations given for either position. The rejection of innate ideas is not included since the focus of this project is ultimately on current more general philosophy of science, and the topic of innate ideas is not relevant to the issues addressed. The second formulation is not included because it is too crude; although it captures the spirit of EE, there are subtleties that it doesn't allow for. To give an example, one criterion that almost all forms of EE adhere to is that of some sort of epistemic scepticism, whereby certain knowledge is rejected. To formulate EE in such a manner as being based around universal statements about "knowledge" is thus to neglect this important component of EE. Two notable examples are Hume and van Fraassen.

Further, a difference has often been pointed to between empiricism *about meaning* and empiricism *about knowledge*, which should be highlighted. Bennett (2002) makes this distinction, but it originally arises through Hume. To be an empiricist about meaning is to assert that 'the limits on what one can understand or make sense of are set by the limits on what one has experienced'. (Bennett, 2002, p98). Under this view, one cannot understand what, say, a black-hole is until one properly experiences it. This sort of empiricism is not one that concerns this project, as it is not a form of empiricism that any current empiricist philosopher of science that I know of would hold to. Empiricism about knowledge fits into the version of EE which I will shortly elucidate.

Experience

Before discussing more clearly what both EE and ME are, something about "experience" should be said. Experience is extremely important to both versions of empiricism, although they each use it and appeal to it in quite different ways. The question of what experience *is*, exactly, cannot be settled here. The analysis here will be purposively vague to allow for various

[3] The name here is taken with slight modification — he calls it "epistemological empiricism" — from Allen (2021). But what the name refers to is really nothing new, and I take it to be simply how one would usually understand empiricism.

possibilities, but will nonetheless aim to give sufficient understanding, and to eliminate some types of experience from what is at stake here.

The typical way of conceiving of experience in standard conceptions of empiricism is of experience being just our sensory perception of the world. Experience is here simply what we observe, and to verify something by experience thus means that one has seen this phenomena. A modification of this notion of experience could be extending it so that instruments of different sorts that serve as enhancers and extensions of our senses and incorporated into the realm of what counts as experience. A hearing aid, binoculars, and various scientific instruments such as an electron microscope or a cloud chamber would function as tools that allow us to extend our experience, under this view.

We can also turn to the term “empirical” to help this analysis, which is based on the term “experience”, to help gain some more insight into the term. Empirical evidence is a commonly used term in the sciences. Empirical evidence is evidence that both falls within a theory or model’s given domain, and is directly observed or detected by instruments. “Observed” is used in an extremely broad sense of the term, and scientists typically use the term in a very different manner to philosophers; the scientist’s usage rarely depends on sensory experience in the way that it does for philosophers (Shapere, 1982).[4] It is also helpful to contrast this with entirely non-empirical evidence; for instance, various theoretical virtues such as simplicity, explanatory power, unifying power, etc. Understanding empiricism in terms of empirical evidence vs non-empirical evidence is how Boyd (2018) frames the debate about empiricism in current philosophy of science. It will be seen in chapter 2, when looking at the pragmatists William James and John Dewey, that a different conception of experience is given that is not limited to sensory experience but allows for direct experience of various different relations between objects.

There are certain notions of experience that we can eliminate from the understanding of experience for empiricism within the philosophy of science. We surely do experience a wide range of emotions as humans — from happiness to sadness to anger to love, etc — but our experiences of emotion are not relevant to the project of empiricism within the philosophy of science. Whether or not we feel happy or sad or worried or tense when we perform a certain experiment or examine a certain theory is surely not within the realms of scientific practice. But eliminating this notion of experience nonetheless leaves how we conceptualise experience as very open. This consequently leaves it

[4]See chapter 4, section 3 for more discussion on this.

open as to how we conceptualise empiricism itself.

Leaving aside the question of the *nature* of experience, we can briefly explore the *functional role* that experience plays in empiricism in order to examine experience from a different angle. In the differing versions of EE and ME, experience serves several different functions. It is here, and it is thus through a functional analysis of the concept, that things are made clearer. Experience can serve as a (i) *justification* of belief, as (ii) a *limit* of belief, as (iii) a way in which we can find out about the world. The first two concern beliefs, the third concerns methodology. EE is largely focused on (i) and (ii), whereas ME is largely focused on (iii). But as will be seen throughout the first three chapters, there is overlap.

(i) Experience as Justifying

Positive: For a belief to be justified, the justification must appeal to what can be experienced.

Negative: Supposed knowledge that does not appeal to experience for its justification is not knowledge since the justification cannot be sound.

(ii) Experience as Limiting

Positive: What we can know is limited to what we can experience.

Negative: We cannot know anything beyond what we can experience.

(iii) Experience as methodology

Positive: We should only turn to experience (via empirical investigation) to learn about the world.

Negative: We should not turn to anything outside of experience, such as a priori speculation or assumptions, to learn about the world.

One can endorse (i) without endorsing (ii). One can, for example, hold that we have knowledge of unobservable entities and structure, but base this purely on empirical evidence. One would arrive at the view that such phenomena exist through some form of *inference to the best explanation*. Note though that one could very plausibly endorse belief in what are usually thought of as "unobservable" entities and endorse both (i) and (ii). If one's conception of experience extends to what can be measured via scientific instruments — and thus one conceives of scientific instruments as allowing for an extension of our experience — then it is perfectly coherent to say that (a) electrons exist, (b) that my belief in electrons is justified by experience, and that (c) my beliefs are limited to what we can experience.[5].

The converse of the above is not true. One cannot endorse (ii) without endorsing (i). If an empiricist is questioned about *why* they endorse this sceptical component — that what we can know is limited to our experience — then they must invoke a response resembling something like: "because to have knowledge, or for our beliefs to be justified, this justification must appeal to experience". Thus, the justification component is fundamental insofar as the sceptical component presupposes its endorsement but not vice-versa.

Regarding (iii), this also heavily depends of course on how one understands "experience".(iii) is the role that experience plays in the ME tradition, and within ME experience is not limited to sensory perception. To turn to experience to investigate the world, on most accounts, is then to use any tool or instrument at our disposal in order to perform this investigation.

1.1.1 Epistemic Empiricism

EE can be characterised by several features detailed below. These are not necessary and sufficient, either individually or jointly; they should instead be seen as a more "property cluster" style characterisation of EE. As stated earlier, with EE, experience is focused on epistemic issues. It plays the role of either the *limits* of our knowledge and/or is used to *justify* our knowledge. EE also typically gives sensory perception special epistemic status.

Characteristics of Epistemic Empiricism

(1) Individualistic: Experience is considered from the perspective of the individual human observer.

(2) Centrality of Sense Perception: Experience and observation is construed in terms of sensory perception.

(3) Epistemic Scepticism: EE is heavily influenced by scepticism of various epistemic kinds.

(4) Theory-Led: EE is based far more theoretically than practically (what

[5] See chapter 4 for more discussion on this

works in theory over what works in practice), and in philosophy of science based far more on scientific theories than on scientific practice.

(5) No Metaphysics: EE rejects unobservable structure or entities, where the unobservable is demarcated with reference to the human observer.

The epistemology of EE is a *theory-first* epistemology. It is not guided by science or by practical investigation, but guided by reasoning, philosophy, and theory. This is a consequence of (4) — matters are considered in light of abstract, theoretical reasoning rather than via practical investigation. EE is concerned with what happens in theory. For instance, Hume asserts that we can't rationally justify induction and thus can't rationally justify claims such as the claim that the sun will rise tomorrow.[6] This conclusion is arrived at through abstract, theoretical contemplation, and not by observing with what frequency the sun actually does rise, or considering various other factors about the lifetime of stars and making a prediction based on this.

(1), (2), and (4) place EE in a Cartesian tradition in some sense of philosophy being the contemplation of an individual examining and reflecting on their own experiences and thoughts, and trying to work out what is and is not the case through this.

(3) — the influence of epistemic forms of scepticism — is seen most prominently in Hume, but is prevalent in most EE, especially of the Early Modern period (as shown below).

(5) deserves some clarification. Important pre-Humean empiricists in this tradition would discuss what we would now think of as metaphysics. Examples include Locke's causal principle and discussion of "powers". Whilst those in the earlier EE tradition certainly rebel against the more inflationary metaphysics of the dominant Aristotelian paradigm of their time, they cannot be said to have removed metaphysics entirely. It is thus post-Hume that we really see this complete eradication of metaphysics emerge in EE, although arguably it is there "in spirit" prior to Hume. This criterion is included since the focus of this project is on empiricism in current philosophy of science, and empiricists in the EE tradition certainly satisfy this criterion in current times.

[6] See section IV of *An Enquiry Concerning Human Understanding*.

1.1.2 Methodological Empiricism

The division between these two styles of empiricism is not novel. For example, it is commonly understood that Francis Bacon is an empiricist of a different sort to the slightly later empiricists of the Early Modern period. In current philosophy of science Nancy Cartwright locates her own empiricism in an empiricist tradition that was 'uncontaminated by the Cartesian doctrine of ideas' (Cartwright, 1989, p3), citing Glanvill, Boyle, Hooke, Power, Bacon, and Willliam Thomson (Lord Kelvin) as being empiricists with whom she sees herself aligned with. Allen (2021) refers to many thinkers in the category I label ME as belonging to a style of "experimental empiricism" (Allen, 2021). This is an empiricism that prioritises experiments and emerges with Grosseteste and becomes most prominent in Bacon. There is much overlap between this category and ME, but also there are important differences.[7]

In ME, experience plays an absolutely central but importantly different role. The role that experience plays in ME, as stated above, is as a *methodology*. Turning to experience is the way in which we find out about the world, and empirical investigation is how we should proceed when investigating the world. As will be seen, experience is typically construed broadly enough to allow for whatever tools and instruments that our science of the time permits.

Characteristics of Methodological Empiricism

(1): Empirical Investigation. The focus is still on experience but on using experience to investigate the world. An empirical/empiricist method is the best way to proceed.

(2): De-centralisation of sensory perception. There's a general shift away from a focus on sensory perception, and so consequently a lack of focus or dismissal of the significance of the "observable-for-humans". The focus is instead far broader.

(3): Mild scepticism. There exists some sort of scepticism, but typically

[7]For example, his focus on experiments and categorisation as such means that the pragmatism of Dewey and James does not fit into this version of empiricism, whereas I argue in chapter two that these thinkers are important figures in the ME tradition. He introduces a different category of empiricism for Dewey and James, which he takes from James — "radical empiricism" (Allen, 2021, p289-305). Whereas he sees this as significantly different to the empiricism of Francis Bacon and the experimentalists of the Early Modern era, I argue that there is significant enough continuity to classify them as existing within the same version.

far less radical than that of the epistemic version. It tends to be local and not global, and not so severe as to reject the possibility of any metaphysics or notion of knowledge.

(4): Practice-led. The focus is far more on scientific practice rather than scientific theory, and on what works in practice rather than what works in theory.

(5): Willingness to endorse some metaphysics. The kind of metaphysics that empiricists of ME endorse is something like a purely naturalistic metaphysics that is justified by directly appealing to scientific experiments or practice.

(6): Shift away from philosophy-first epistemology.*Philosophy-first* epistemology does not play a central-role; any epistemology done is justified through the empirical method.

Regarding (2), the focus of EE on sensory perception is replaced with something far broader. For instance, Bacon and the Baconians of the Royal Society focus heavily on learning through experiments and instruments, and would assert that these experiments are really telling us something about the world despite the fact that causes cannot be observed with the naked eye (see 1.4.1). Cartwright's focus is on what can be learned through scientific practice and through scientific investigation (3.2). Dewey and James argue that experience is far more encompassing that sensory perception, and that phenomena such as relations can be genuinely experienced in a significant sense.

Central thinkers can be turned to to discuss (3). There are elements of scepticism in Bacon's philosophy, but it is restricted to scepticism about various phenomena and methods that he rejects from his philosophy, and not a general scepticism about claims to knowledge.[8] Boyle is certain of "matters of fact" but holds scepticism towards the certainty of causes and other less secure elements of knowledge.[9] Glanvill believed that we can't definitively establish our senses as reliable, and any attempt to demonstrate their reliability would have to employ the senses (Popkin, 2003, p210). Cartwright argues against large leaps in ampliative inferences on sceptical grounds.[10] All of these points will be elaborated on in what follows.

[8]See the explication of Bacon's empiricism in 1.4.1 of this thesis.

[9]See the explication of Boyle's empiricism in 1.4.1 of this thesis

[10]See 3.2 of this thesis, where Cartwright's inferential conservativism is discussed.

(4) naturally falls out of (1): if the stress and focus is placed onto empirical investigation of the world, then it follows that we turn to practice rather than to theory. There is an important clarification that should be made here, especially with respect to the "theory-led" criterion within EE There are two components here that are slightly different but highly complementary. The theory-led criterion encompasses both a focus on what works in theory and a focus on theories in science, and the practice-led criterion encompasses both a focus on what works in practice, and a focus on practice in science/scientific practice.

(5) has potential to mislead, and is somewhat anachronistic when applied to earlier thinkers such as in the Early Modern period. The term "metaphysics" used here is understood in the way that current philosophy of science and at least analytic philosophy uses it more generally. Ontological claims about reality, both observable and unobservable, are metaphysical statements; as are claims about causality.

1.2 Ancient Proto-Empiricism

In Ancient Western philosophy there is not a fully-formed empiricism. There are, however, philosophers and schools that have various empiricist characteristics. There are also certainly philosophies in this period that can be characterized as being broadly empiricist in their method of approaching problems. It's beneficial to examine these characteristics in order to have a better understanding of where the more modern forms of empiricism come from, especially since some of the more modern empiricists explicitly align themselves to these philosophies

The Hippocratic Physicians

Hippocrates (460- 370 BCE) and the Hippocratic physicians are the earliest example in Western philosophy of something resembling empiricism, or at least an empiricist method. As Allen (2021) notes, texts from Hippocrates such as *On the Nature of the Child*, and *Airs, Waters, Places* record both various observations on chicken eggs and describe an experiment that attempts to prove that after water is frozen, it loses its "lightest" part (2021, p12). Observation drives their methodology and their claims as to what is or is not correct. There is no indication that they rejected entities or phenomena beyond what they could observe.

Aristotle

There is motivation to deem Aristotle's (384-322 BCE) philosophy as some sort of proto-empiricism in certain respects, but in other areas he certainly is not. He insists on the reliability of the senses, and on empirical investigation as being an important way to proceed; and he rejects more grandiose metaphysical claims that are not based on empirical investigations, such as Plato's theory of the forms and generally making less speculative metaphysical claims than does Plato and the Presocratic predecessors.[11] But he nevertheless incorporates a metaphysics that no empiricist would endorse — his fourfold theory of causation, being *qua* being, and his hylomorphist accounts of matter.[12]

The parts of Aristotle's philosophy that do fit into empiricism fit into the version of ME far more than they do into EE. We see passages in his works such as: 'credence must be given to the direct evidence of the senses more

[11] For a rejection of Plato's theory of forms, see his *Metaphysics*, book Beta, chapter 4.
[12] see his *Metaphysics* and his *Physics*.

than to theories — and to theories too provided that the results which they show agree with what is observed' (Generation of Animals, 760b)), demonstrating a practice-based approach that focuses on what actually occurs as opposed to what occurs in theory. Aristotle's work on biology puts into action a methodology that focuses heavily on practice, with a great collection of facts and observations obtained and analysed.

1.2.1 Hellenistic Period

The Pyrrhonists

The Pyrrhonian sceptics are absolutely vital to current understandings of empiricism in that, as stated above, *some* scepticism is seen in all empiricism. Whether more dilluted in the ME version, or far more radical in the form of Gassendi, Hume, or the EE tradition more generally, this influence is pervasive. The tradition begins with Pyrrho (360-275 BCE) and is shortly afterwards developed by his student Timon (315-225 BCE) (Vogt, 2018). The main access that we have to this school is through Sextus Empiricus' *Outlines of Scepticism*, which was written several hundred years later.[13]

According to Sextus' account, the Pyrrhonists saw themselves primarily as (i) investigating, and (ii) suspending judgement on philosophical questions (Sextus Empiricus, 2000, p4). Regarding (i), investigation, Sextus presents the pyrrhonists as the middle-ground between the *dogmatists* and the *academic sceptics*. Whereas the dogmatists investigate various forms of nature and claim that they have discovered something, and the academic sceptics will deny any possibility of a discovery, the pyrrhonists reject both and argue that the investigation should be on-going, as we can never definitively reach an outcome that is beyond doubt. Whilst this characterisation from Sextus of the academic sceptics is likely unfair, it does capture an important attitude of empiricism — demanding that with respect to the majority of investigations, the final word has not been said.[14]

[13]The date is hard to pinpoint, but seems to be around late 2nd century CE, according to Annas and Barnes (2000, pxii).

[14]There are of course exceptions to this open-mindedness: for instance, it would be unwise or perhaps unheard of for empiricists to deny the notion that our best-tested theories in science are approximately empirically correct. But this should be seen as falling into the same category as the Pyrrhonists accepting that the sensory experience that we have certainly does appear to us, but refusing to comment any further — in both instances, we are sure that (a) the experiments performed that have confirmed these

Regarding (ii), Sextus argues in favour of suspending belief with regards to claims that have conflicting evidence, instead opting to not question the appearances that we see and taking them as they are:

'When we say that the sceptics do not hold beliefs, we do not take "belief" in the sense in which some say, quite generally, that belief is acquiescing in something; for sceptics assent to the feelings forced upon them by appearances — for example, they would not say, when heated or chilled, "I think I am not heated (or: chilled)'. Rather, we say that they do not assent to some unclear object of investigation in the sciences; for pyrrhonists do not assent to anything unclear'.

(Ibid, p6).

All of this was motivated by a practical concern — the goal of their suspension of belief was to attain *ataraxia*, a state of tranquillity that arises from the stopping of worrying about philosophical issues (ibid, p5).

The pyrrhonists have had incredible influence over essentially the whole of western philosophy from the time of their rediscovery, but especially over empiricism.[15] The rejection of (a) speculative metaphysics, (b) sources of authority, and (c) appeals to things that exist beyond our sensory experience are an incredibly important part of many forms of empiricism that follow them. The insistence on practical investigation is also in accordance with elements of ME.

Epicurean Empiricism

Epicurean empiricism emerges around a similar time to the pyrrhonists, with the school founded by Epicurus (341 - 270 BC). Epicurus and the Epicureans held both that observation is the only source of knowledge that one can have about the world (Lipton, 2001), and also that all of these perceptions are true (Konstan, 2018). The latter point sharply separates their view from the sceptics. Licensed from their view that sensory experience is entirely accurate and thus gives us knowledge about things, the Epicureans held also to a doctrine of atomism that was inspired by Democritus and Leucippus but modified in light of criticisms that faced the earlier atomists. The brand of empiricism here therefore endorses the justification component of experi-

theories have produced empirically correct results; and (b) that our sensory experience is certainly appearing to us in some way. But in both cases judgement could be reserved as to whether or not this reflects reality beyond our experience.

[15]See section 1.4 for more on this

ence but rejects the limiting nature of experience. Epicurean philosophy was hugely influential for Gassendi — who will be discussed later in this chapter — who wrote two books on the topics and considered himself an Epicurean.

The Empirics

The first to actually describe themselves as empiricists of some variety were medics from the Hellenistic period, who called themselves 'empirics' or 'empiricists'.[16] The empirics relied heavily on philosophical scepticism, and later philosophers of the empiric school included Menodotus and even Sextus Empiricus (hence the name), who were both heavily involved in the pyrrhonist school of scepticism (Morrison, 2019). Parts of their philosophy have much overlap with important elements of EE that we see from Hume onwards: they explicitly refused to posit causes and entities beyond what can be observed, and believed generally that the limits of our sense experience are the limits of what we can know. This theoretical framework was, as noted by Allen (2021), directly applied to their medical practice. The empirics were incredibly conscious of the limits of their knowledge and general practice of medicine.[17]

Pomata (2011) documents how the term "observation" as a philosophical term first emerges with the Empirics, and was further developed by the sceptics (2011, p1). The concept of observation is vital to EE. However, the way in which it was used by the Empirics included all the senses, and not just sensory perception as we commonly associate it now (ibid, p5).

Cicero

Beanato (2020) documents how Cicero (106-43 BCE) rejected genethliac astrology on the grounds of it not being scientific. The terms "scientia" is clearly different to science as we conceived it now, but the essence of the claim is still significant in that he is rejecting what we take to be an exemplar of pseudo-science (astrology) as not being a genuine form of knowledge. Beanato (ibid) further lists several criteria of what Cicero argues constitutes

[16] However, referring to ones self as an empiricist should not really be seen as endorsing what we collectively now understand empiricism to be. The most paradigmatic empiricists — "classical" or "British" empiricists — certainly didn't refer to themselves in this way, and philosophers that we now see as empiricist such as Bacon rejected a label of empiricism and wrote about this negatively.

[17] See: 'This inaugural empiricism is also the first notice in European science of the value of tentativeness and caution, admitting doubt and acknowledging mistakes, qualifying generality, with express wariness about unsupported claims' (Allen, 2021, p24).

science, which bear a remarkably similarity to modern accounts of science. Amongst those, most significant here regards his attitude to empirical investigation. Cicero argues that astrology does not constitute science because it does not make predictions that are empirically accurate (ibid, p100). He saw astrology as being empirically falsified a large number of times, where predictions had been made and had not come true.

1.3 Medieval Proto-Empiricism

The Middle Ages/Medieval period is often seen as beginning in 312 AD with the conversion of the Roman emperor Constantine to Christianity, and ending in the 1520s/1530s with the reformation and split from the catholic church (Luscombe, 1997, p2). Whilst this period exists in mainstream imagination as an era of superstition and backwardness, there were many practical advances in the Medieval period which showcase significant engineering improvements. For instance, from as early as the 9th century the Saxon wheeled plough and an improved system of crop rotation had come to be used in Northern Europe (Crombie, 1953, p16); various methods and techniques had been developed to build watermills and windmills (ibid, p17); mining and metallurgy was mechanized which made possible the productions of metals on a large scale (ibid). The High Middle Ages, specifically around the 12th century, marked the rediscovery of Aristotle's work. And from this period until the end of the Medieval era, Aristotle was the central authority to appeal to within philosophy.

This section touches on influences of current empiricism in philosophy of science in two ways: the first is via the beginnings of philosophical focus on experimentation which fits into the version of ME, and the second is via the Medieval school of nominalism, which both fits into the version of and provides undoubted influence for EE.

1.3.1 Methodological Empiricism

Grosseteste (approx 1170-1253) was essentially an Aristotelian, but departed from Aristotle in certain important areas. Perhaps most importantly is that whereas Aristotle sees within his view of science the potential to arrive at certain knowledge regarding first principles, Grosseteste holds a more sceptical attitude towards this. He claimed that there may be several causes that at least appear to cause the same effect.[18] Going on Crombie's (1953) reading of Grosseteste, when we are trying to find what the correct cause is for a particular effect, we need to rely on experimental verification and some sort of falsification.

'On the basis of these consequences controlled experiments were arranged by

[18] See (Crombie, 1953, p81, quoting from Grosseteste's commentary on Posterior Analytics, Book 2, page 5).

which false causes could be eliminated. The investigator could then entertain as the true cause that which was left after all the other possibilities had been eliminated'

(Crombie, 1953, p83).

Whereas Aristotle's focus was on appealing to everyday experience to justify beliefs, if we are to use Crombie's reading of Grosseteste, then Grosseteste appealed to some form of experiments to justify beliefs rather than everyday sense experience (Allen, 2021, p111). He was also the first Medieval writer to systematically analyse and discuss mirrors, lenses, and the rainbow (Allen, 2021, p113) (Crombie, 1953).

Grosseteste's work heavily influenced the subsequent generations of Oxford philosophers, which was where he was educated and lectured. The philosopher who was most influenced by Grosseteste, and who developed Grosseteste's philosophy the most, was Roger Bacon (1219-1292). Bacon, like Grosseteste, develops his philosophy via a commentary on Aristotle's *Posterior Analytics*. In a similar style to Grosseteste, Bacon asserts that reasoning alone cannot provide us of knowledge but that experience needs to be introduced.[19]

Roger Bacon believed that "experimental science" has three great prerogatives with respect to the other sciences. The first is that it investigates the conclusions of all the sciences — i.e., experiments should be used to confirm conclusions (ibid). The second is that the experimental science should add new knowledge to existing sciences (ibid, p142), and the third is to create entirely new departments of science (ibid). In Bacon, also, the term *scientia experimentalis* first appears in part six of his *Opus Majus*. This section is a study of experimental science which takes optics as its model (Hackett, 2020).

Kupfer (1974) argues that Roger Bacon, as opposed to Francis Bacon, is the true founder of Early Modern empiricism, and discusses how Roger Bacon's emphasis on experiment transferred across to Francis Bacon. The former, in his *Opus Majus*, details four obstacles to truth acquisition that are similar to the latter's "four idols" that will be seen in the subsequent section.

[19](Crombie, 1956, p141)

1.3.2 Epistemic Empiricism

Nominalism, like EE, rejects speculative and metaphysical thought; it opts instead for a focus on the empirical realm. Reacting to advocates of "universals" who claim that concepts such as red, goodness, or pain are actual entities that really exist external to the humans, the nominalists argue that the extent to which these exist as universals is in name only. One will see many instances of red, or goodness, or pain, and one will use these terms as categories to group various similar things together. In summarised and alternative wording, the project of the nominalists can be seen as reducing claims about concepts *from* grandiose metaphysical claims *to* linguistic claims of predication. Typically, the two most significant figures within explications of Medieval nominalism are Peter Abelard (1079-1142) and William of Ockham (1285-circa 1347), from each of the two periods of Medieval nominalism. What the nominalists do to concepts they do also to relations: relations are seen *not* as real entities but as linguistic constructs.

Ockham is particularly interesting as several commentators — for instance, (Gilson (1950))(Klocker, 1960)(Grant, 1978)(Moody, 1975) — have interpreted him as being an anti-realist about causality, thus effectively pre-empting the Humean and post-Humean forms of EE. It should be noted, however, that this is not a universal understanding of Ockham; for instance, Adams (1979) argues that all of these views have over-generalized certain parts of his writings to claims about his overall epistemology and metaphysics. And dismissing this more sceptical interpretation of Ockham, Allen claims that Ockham 'has no patience for skepticism' (2021, p130), and quotes an anonymous scholar as writing that Ockham has 'unshakeable conviction in the possibility of certain knowledge and ... a natural universe so ordered as to assure that possibility' (ibid).

1.4 Early Modern/Classical Empiricism

It is in this period that empiricism properly emerges, and standard accounts of empiricism — as stated before — will typically point to Locke, Berkeley and Hume as being the most important figures in the empiricist tradition. Out of all of the philosophers discussed in the historical component of this chapter, Francis Bacon and Hume are the most influential for current manifestations in philosophy of science of both ME and EE, respectively. Consequently, a more comprehensive exposition of their empiricism is given.

As Popkin (2003) and Woolhouse (1988) document, the Early Modern era was hugely influenced by the rediscovery of Sextus Empiricus' "Outlines of Pyrrhonism". And more generally this era is one marked by a general scepticism — a doubting and questioning of figures of authority and traditional dogma that can be seen in, for instance, Luther's rejection of the Papacy, the birth of Liberalism, and enlightenment thinking. This scepticism is displayed in both ME and EE, although to a greater degree in the latter.

To an extent, the empiricism in this period has the sceptical rejection of Aristotle's metaphysics at its core. Or more accurately, a rejection of the dogmatic way of thinking that became entrenched in the Medieval period in the Aristotelians who thought of Aristotle as the supreme authority on everything. The earlier empiricists — Francis Bacon and Pierre Gassendi — are far more vocal about it, publishing texts explicitly aimed at rejecting Aristotle's philosophy and against the Aristotelians. [20] The empiricists who come later in time are still entirely opposed to Aristotelian metaphysics, but they typically show this implicitly by formulating their own philosophies that stand firmly against the Aristotelians rather than explicitly vocalising their opposition as vehemently as Bacon and Gassendi. This is due to the dwindling adherence to Aristotle over time as "experimental philosophy" became more popular (Anstey, 2005)(Anstey and Vanzo, 2012). And by the time that we get to Hume, whose philosophy is most radically at odds with Aristotle out of the Early Modern empiricists, Hume mentions Aristotle only twice in *An Enquriy Concerning Human Understanding*, and once is to declare the Aristotelian philosophy essentially dead.[21]

[20]Bacon's "Novum Organum", which is supposed to replace Aristotle's "Organum", and Gassendi's "Exercises in the Form of Paradoxes Against the Aristotelians".

[21]'The fame of Cicero flourishes at present, but that of Aristotle is utterly decayed' (Hume, 2007, p4).

Earlier in this chapter, Aristotle was characterised as a proto-empiricist of some sort. It may be asked how this squares with what has just been written. To emphasise — what was being rejected in the Early Modern period of empiricism was the more speculative metaphysics of Aristotle's philosophy, which was emphatically *not* empiricist in any way, and also the dogmatic adherence to Aristotle from the Aristotelians. The elements of Aristotle which were above classified as proto-empiricist in the ME tradition were that of empirical investigation into the world, and an emphasis on learning from experience.

1.4.1 Methodological empiricism

Francis Bacon

The main text where Francis Bacon (1561-1626) puts forward his ME is in his *Novum Organum* (1620)(Henceforth NO). Two important things stand out from the title of this text. The first is the term itself - in English, it means "New instrument" or "New tool". Bacon conceived of this text as a guide that presents us with the correct method/instrument/tool to interpret the world and thus to gain knowledge about the world. The second is contextual: it is a direct reference to Aristotle's "Organon". [22] The point of the text is thus to replace the old Aristotelian method of doing science/gaining knowledge with the new, better Baconian method of doing science/gaining knowledge.

The NO is divided into two books. The first is mainly negative and destructive, and is concerned with both refuting the old method of gaining knowledge of the world via speculation and uncritical sensory experience. He is critical of both the sophistical approach that he associates with Aristotle, and the overly-empirical approach that he associates with Gilbert. The former leads to making vast generalisations from very small amounts of evidence, and the latter leads to limiting all knowledge only to the data that one has. In the first book he talks about the now-famous "idols", which are four-fold. The first is the idols of the tribe, which involves errors in perception, caused by the limitations of the human sense (Bacon, 2000, p40) The second is the idols of the cave, which arise from each individual's personal prejudices and attachments to particular styles or modes of explanation (ibid, p41). The third is the idols of the marketplace, which arise directly from the way that language is used (ibid). Finally, we have idols of the theatre. This is to do

[22] "Organum" is the latin for the Greek word "Organon", hence the slight change.

with mistaken methods and philosophies which have led people astray (ibid, p42).

Book 2 deals with Bacon's positive philosophy, and introduces his method which he believes that if followed correctly, we can overcome the four idols and come to have knowledge. The knowledge that Bacon is concerned with is knowledge of the nature of phenomena and knowledge of causes. Note that the idea that natural philosophy should be the investigation of causes is nothing new. Aristotle does this, as does Medieval philosophy; thus Bacon rejects the Aristotelian idea of producing syllogistic arguments from first principles or axioms, but retains its aim of achieving knowledge of causes. And it is formal causes that he is worried about, or "form", and terms these 'natures'.

Bacon's positive method has two stages. The first is the "natural history" stage, and the second is the inductive stage. The first involves compiling three tables on the basis of large amounts of observation. The first table concerns the presence of a phenomena, p; the second table concerns the absence of a phenomena, p; and the third contains the correspondence/quantitative change so that we can compare p from one instance to another. The second, inductive, stage has as aim to find a form that is always present or absent with the given nature, and always increases or decreases with it. The induction arises in linking the correlation of this form to a causation. Bacon's natural histories were a core part of his empiricism and his philosophy, and throughout his philosophical career Bacon provided extensive natural histories of various phenomena.

Bacon evidently advocates a method of **empirical investigation into the world** whereby one uses experience to investigate the world and that empirical investigation is the only way to gain knowledge of the world. The very first aphorism of the first book attests to this: 'he [Man] does and understands only as much as he has observed of the order of nature in fact or by inference; he does not know and cannot do more' (ibid, p33).

His empiricism is one that is **practice-based**. For instance, he praises the philosophy of the 'earlier Greeks' (Bacon, 2000, p27) who focus on practice and investigation of the world itself rather than on theory. For instance, he writes that they didn't argue the point on whether or not anything can or cannot be known, but instead decided to 'try it by experience' (ibid). The latter point is an advocation of what works in practice over what works in theory: instead of theoretical contemplation on the matter, we should turn to the empirical consequences and see what actually happens in practice. Bacon also says that knowledge should be pursued for practical purposes, and for the benefit and use of life. This fits neatly into the practice-based element of ME and also into Cartwright's desire for a science and philosophy that benefits society. He writes that '[t]he true and legitimate goal of the sciences is to endow human life with new discoveries and resources' (ibid, p66).

He **invokes some sort of metaphysics**. He thinks that, through his method, we really are investigating the world itself and not just our perception of the world. It is what would now be referred to as scientific realism. He also frequently talks about genuine knowledge being gained of causes within the world. One example of this is his philosophy of experiment. Here, he discusses"luciferous", or "illuminating" (depending on the translation) experiments, which are experiments that allow us to discover real causes about the world (ibid, p81). These are contrasted with "fructiferous", or "profitable" experiments (ibid) (again, depending on the translation), which only give us practical use and don't discover the world for how it really is. As will be seen below, Boyle takes up this idea also.

Bacon **argues against the primacy of sensory experience**, with the priority going instead to careful, repeated observations, or experiments and machines, to learn about the world. He says in the preface that his method must be constantly controlled and aided by machines (ibid, p28), and the idols of the tribe also attest to this. Perhaps the clearest place where he talks about this is the following:

'[M]uch the greatest obstacle and distortion of human understanding comes from the dullness, limitations and deceptions of the senses; so that things that strike the senses have greater influence than even powerful things which do not directly strike the senses... by itself sense is weak and prone to error, nor do instruments for amplifying and sharpening the senses do very much. And yet every interpretation of nature which has a chance to be true is achieved by instances, and suitable and relevant experiments, in which sense only gives a judgement on the experiment, while the experiment gives a judgement on nature and the thing itself'.

(ibid, p45)

He also **invokes some sort of localised scepticism**, but certainly doesn't advocate any sort of global scepticism. For the former, recall above the four idols that he discusses which are all instances of scepticism towards various different ways that the mind appears to have knowledge. And he invokes a scepticism towards the Aristotelian methodology of gaining knowledge, also. For evidence of the latter, in the preface to NO he argues that his method allows for 'sure, demonstrable knowledge' (ibid, p30), whereas alternative methods from ancient philosophy allow for 'nice, plausible opinions' (ibid). He also writes that his method allows us to 'construct a new and *certain* road for the mind from the actual perceptions of the senses' (ibid, p28, my emphasis).

Boyle

Boyle (1627-1691) fits into the tradition of the 17th century experimentalists, and was one of the most important figures in developing the science of experiment in the Early Modern period. He was a founder of the Royal Society, which was instrumental in advancing the new experimental science to a broader audience. Boyle was a Baconian at least to some extent (Krook, 1955)(Anstey, 2014), but there are important differences between the two, which will be highlighted below.

Boyle's empiricism matches all the features characteristic of ME. It is **led by empirical investigation** and firmly **practice-based**. Boyle gave epistemic priority to observation and experiment over theory; Boyle existed within the tradition of experimental philosophy which conceived of theory being formulated after the experiment or observation had taken place (Macintosh and Anstey, 2018).[23] Boyle, following Bacon, was also heavily invested in the production of the aforementioned "natural histories", and using them as a method of producing knowledge (Anstey, 2014, p110).

There is similarly **no philosophy-first epistemology** in Boyle's work. There is epistemology, but it is driven by experiment and practice over philosophical first-principles. An important difference between Bacon and Boyle is with respect to the degrees of their scepticism. As seen above, Bacon believes that we can have certainty in science and that this certainty is quite liberal. Boyle was more sceptical. This is perhaps not overly surprising, given that around the mid 17th century there was a general shift towards a more sceptical attitude that no longer saw science as certain and demon-

[23] See also: 'Robert Boyle maintained that proper natural philosophical knowledge should be generated through experiment and that the foundations of such knowledge were to be constituted by experimentally produced matters of fact' (Shapin and Schaffer, 1985, p22).

strative, but inclined more towards a probabilistic conception of knowledge (Shapin and Schaffer, 1985, p23-24). Boyle's scepticism follows in the vein of other founders of the Royal Society, namely Wilkins and Glanvill (Popkin, 2003, 208-219). Boyle takes this type of scepticism to oppose what he sees as the overly dogmatic theories of Aristotle, Descartes, and Bacon (ibid, p217). But Boyle did believe that we can have certainty in science, but that this certainty is limited to "matters of fact"; the certainty that Boyle believed we could have did emphatically not, at least in theory, extend to notions such as causality which Bacon would endorse.[24] "Matters of fact", were empirical facts that can be known through experiment and, to a lesser extent, naked eye observation.

A "matter of fact" is a fact that we obtain through experiment or observation, and sits at the foundation of scientific knowledge (Shapin and Schaffer, 1985, p22). An example is Boyle's law, $PV = K$ — or, qualitatively, that the absolute pressure exerted by a mass in question is inversely proportional to the volume it occupies if the temperature and amount of gas remain unchanged within a close system. Matters of Fact could only be established through criteria of reliability, which included the scientific instrument being used being up-to-scratch (ibid, p29), and collective acceptance within the scientific community that required witnessing the fact through experimental means:

'Members of an intellectual collective had to assure themselves and others that belief in an empirical experience was warranted. Matters of fact were the outcome of the process of having an empirical experience, warranting it to oneself, and assuring others that grounds for their beliefs were adequate'

(ibid, p25).

Boyle also **invoked some metaphysics**. He argues in "A Disquisition About the Final Causes of Natural Things" that there are "final causes" and that in many cases we can have epistemic access to said final causes. Boyle also, again following Bacon, favoured luciferous experiments over fructiferous experiments. The former allowed the experimenter to gain knowledge about the causes of the phenomena, whereas the latter yielded purely practical outcomes (Anstey, 2014). And Boyle, along with Bacon and Hooke, believed

[24]Shapin and Schaffer (1985, p50) document how Boyle, although not in theory endorsing causes as certain, would often in practice identify certain causes as "matters of fact" and thus endorse some causes as certain. The point remains though that in principle Boyle was opposed to giving the status of certain knowledge to causes.

that the experiments that they were performing were really interacting with nature itself, and not just with our perception of nature (ibid). This can be seen from the above discussion of matters of fact. This may seem at odds with what has just been written on matters of facts and Boyle's scepticism. Shapin and Schaffer (1985, p50) discuss how Boyle would often identify certain causes as matters of fact, whilst others would be given mere practical value. Whilst there is maybe a tension that can be drawn out, it nonetheless does appear that Boyle held to a notion of causal realism.

Whilst Boyle clearly endorsed some sort of metaphysics, at least by today's standards, the nature of the metaphysics done should be clarified. When characterizing ME above, stress was placed on not wishing to align the metaphysics that ME is happy to engage with with a more speculative metaphysics, and instead point to an entirely naturalized metaphysics. This can be shown well with Boyle's attitude towards a particular experiment. Shapin and Schaffer (1985) call this experiment the "void-in-the-void" experiment, and it was an attempt to elaborate on the Torricelli experiment performed in Pisa in 1643. Torricelli filled a tube with mercury, inverted the tube into a dish filled with a small amount of mercury, and observed that there was a space at the top of the now-inverted tube that had no mercury in it, despite there being mercury in the dish that could move upwards and fill this space. This sparked much debate in natural philosophy and split philosophers into two camps — those who thought that there was a vacuum here, and those who thought that there was not. Boyle expanded on this experiment. The details are not important here — what is important is that Boyle did not believe that this metaphysical question regarding whether or not there was a vacuum could be answered via experimentation, and it was thus *not a suitable question to ask* (ibid, p41-46).[25] Thus the metaphysics that Boyle, Bacon, and ME in general should be seen to be endorsing are assumptions compatible with naturalistic metaphysics, such as that we really are engaging with the world when we experiment, that causality exists and that we can identify causes, that there exist laws of nature, etc. And emphatically *not* the more speculative metaphysics associated more with the scholastic philosophers. To be clear: *all* forms of empiricism certainly reject

[25]See: 'What he was endeavouring to create was a natural philosophical discourse in which such questions were inadmissible. The air-pump could not decide whether or not a "metaphysical" vacuum existed. This was not a failing of the pump; instead, it was one of its strengths. Experimental practises were to rule out of court those problems that bred dispute and divisiveness among philosophers, and they were to substitute [it with] those questions that could generate matters of fact upon which philosophers might agree' (ibid, p46)

this type of speculative metaphysics.

Boyle also **de-centralises the importance of sensory perception**. Boyle believed that scientific instruments both refine and enhance the naked-eye observations that we make. They were preferrable to ordinary observations that we make without instruments.

‘the Informations of Sense assisted and highlighted by Instruments are usually preferrable to those of Sense alone’

(Boyle, requoted from Shapin and Schaffer, 1985, p36).

Hooke

Robert Hooke (1635-1703) follows in the Baconian tradition — he considered himself a follower of Bacon (Hesse, 1966) — that Boyle exists within, and the philosophy here is very similar to these two. Hooke was Boyle’s laboratory assistant, and was responsible for helping to build Boyle’s famous air pump. Like Boyle and Bacon, and in keeping with the typical scientific realism of the tradition of ME, Hooke also sees experiments as really investigating nature itself:

‘The reason of making experiments is, for the discovery of the method of nature, in its progress and operations’

(Hooke, 1726, p26)

We see in this fact several characteristics of ME displayed at once. There is a **focus on empirical investigation**, given that the way of learning about the world is via experiments. We see a **willingness to endorse some sort of metaphysics**, in that Hooke views the process of learning via experiments as really telling us something about nature. With this, and adding in the fact that Hooke believed that the relation between theory and experiment was that experiment came first in knowing about the world, we see that his philosophy is **practice-based**.

Like Bacon, Hooke believed that we can derive axioms from natural histories: discussing his methodology of experiments, he says after we establish the results of the experiments, we should ‘raise such axioms and propositions, as are thereby plainly demonstrated and proved’ (Hooke, 1726, p27).

In Hooke we also see clearly the view that naked sensory experience is not a limit of our knowledge of the world, and thus a **de-centralisation of sensory perception** as per ME. We can see this and the interesting view that experiments and scientific instruments expand the senses in some significant sense in various passages of his work. Pointing to just two:

'[it]was rather to improve and increase the distinguishing faculties of the senses [...] in order to the inlarging the limits of their power, so as to be able to do the same things in regions of matter hitherto inaccessible, impenetrable, and imperceptible by the senses unassisted. Because this, as it inlarges the empire of the senses, so it besieges and straitens the recesses of nature: and the use of these, well plied... will in short time force nature to yield even the most inaccessible fortress'.

(Hooke, 2005, the preface, sig b2)

And

'The next care to be taken, in respect of the Senses, is a supplying of their infirmities with instruments, and ... the adding of artificial Organs to the natural'

(Hooke, 2005, the preface)

1.4.2 Epistemic Empiricism

Gassendi[26]

Gassendi (1592 - 1655) is a neglected but important figure in this tradition. The orthodox narrative typically involves Locke, Berkeley, and Hume, at the expense of Gassendi; although at least since Norton's *The Myth of British Empiricism* (1981), Gassendi has started to be recognised more as an important figure in this tradition.[27] There are three main parts of his philosophy relevant to his empiricism. The first is his anti-Aristotelianism, the second is his scepticism, and the third is his Epicureanism, which becomes developed later on in his life. These will be briefly outlined in turn and tied into his

[26]Note that whilst most historians do denote Gassendi in the Early Modern period, Fisher (2013) argues that there's reason to place him into the tail-end of the Renaissance period instead.

[27]Here, Norton argues that it is Gassendi, not Locke, who is the founder and seminal figure of modern empiricism (Norton, 1981, p334).

EE.

As discussed above, anti-Aristotelianism is a trend that is common to all empiricists in the Early Modern period. Thus Gassendi is not unique in his rejection of this philosophy, although is certainly one of the earliest to do so. His criticisms were aimed largely at the scholastic followers of Aristotle, who followed him dogmatically and uncritically (Woolhouse, 1988, p50). It was therefore aimed at Aristotelianism as the Medeival philosophers saw it. Of most relevance here is his focus on and attack on the conception of "knowledge" that the Aristotelian philosophy had; namely that knowledge (i) was certain and that (ii) it was knowledge of the *natures* or *essences* of things. Gassendi rejects the idea of certainty in knowledge claims and also that knowledge should be about natures or essences. In Gassendi's alternative proposal we see his **focusing on sensory perception**, his **individualistic focus**, and his **epistemic scepticism**: Gassendi advocates (a) an individualistic, probabilistic type of knowledge that cannot ever claim certainty, and (b) that this knowledge be about things that we can experience through our senses (Woolhouse, 1988, p57)(Popkin, 2003, p92). Knowledge is knowledge of what we can experience with our senses; and we should not aim to go further than this into speculative metaphysics — thus satisfying the **no metaphysics** criterion — and nor should we treat this knowledge with any sort of certainty. Gassendi's unwillingness to hold sense experience as certain derives from a scepticism that is taken largely from Sextus Empiricus' Pyrrhonism, but Gassendi does not go so far as the Pyrrhonists do in attempting to suspend all beliefs (Fisher, 2013). Norton describes his philosophy as:

'an attempt to find a middle position between the dogmatism of the Aristotelians and Descartes, and the Pyrrhonistic scepticism of Sextus Empiricus, Montaigne, and others, an attempt which resulted in the formulation of a theory of knowledge suited to the limited, phenomenalistic interests of the new natural science'

(Norton, 1981, p335).

Gassendi was willing to hold beliefs about unobservables as legitimate, at least in his later works. He developed an Epicurean atomism which relied on this. We can know things, through ampliative inferences, that are naturally concealed to us through the limits of our perception. For example, anatomists have inferred that there are tiny holes in human skin, and that the existence of such holes (pores) has been confirmed by microscopic observation (ibid, p335). Importantly, he never moved so far as to argue that these pieces of knowledge were knowledge of the true natures or essences of things, in a more metaphysical sense that the Aristotelians used (ibid), and thus can be said to retain his epistemic scepticism of his earlier philosophy.

Locke

It is in Locke (1632-1704) that we see the most fully fleshed-out doctrine of empiricism, and it is from Locke also that the main stereotype of empiricism arises. That there exists no innate knowledge is typically seen as a central, if not *the* central, component of empiricism, and it is Locke who focuses most on this. Others in the EE tradition discuss this relatively little. The work where his empiricism is treated most extensively and systematically is *An Essay Concerning Human Understanding* (henceforth *Essay*), and it is therefore this text that is the focus here. All references by Locke are made with respect to this text.

The aim of Locke's *Essay* is to 'inquire into the original, certainty, and extent of human knowledge, together with the grounds and degrees of belief, opinion, and assent' (I.I.2). He also makes clear that he is not concerned with grander metaphysical questions of these topics.[28]

Locke devotes the first book of his *Essay* to arguing against innate knowledge through a few different philosophical strategies. A particular focus is on the argument from Nativists that "we have certain pieces of universal knowledge, and so therefore this knowledge must be innate". Locke argues that if there was universal knowledge, universality does not render them automatically innate, and much more evidence must be given than just this. There are clearly other ways by which we could know this hypothetical universal knowledge. But this is besides the point for Locke: in (I.II.4)-(I.II.5) in particular and generally through the whole of the first book, he argues that there is nothing that we can hold to be universal knowledge.

Book two begins the positive explication of his philosophy, and Locke sets out a taxonomy and discussion of *ideas*. Ideas, he explains in book 1, are simply 'whatsoever is the object of the understanding when a man thinks' (I.I.8). He's not concerned with a deeper, more metaphysical explication of the nature of ideas. It is simply a stand-in term for the things that we can

[28]'I shall not at present meddle with the physical considerations of the mind, or trouble myself to examine wherein its essence consists; or by what motions of our spirits or alterations of our bodies we come to have by sensations by our organs' (I.I.2).

think of.[29]

Ideas can be separated into *simple* and *complex* (ibid, II.II.1). The source of all of simple ideas comes either from *sensation* or *reflection* (ibid, II.I). Most of our simple ideas come from sensation, and these ideas have their origins externally from interacting with the world. Some simple ideas though come from reflection; this is the mind reflecting on itself, and ideas derived from here come internally. *Simple* ideas are the "building blocks" of *complex* ideas, and complex ideas are formed through manipulation and combination of simple ideas. Since all complex ideas are compositions of simple ideas and since simple ideas have their source in either *sensation* or *reflection*, these two sources are exhaustive.

A good overview of this particular section, and a demonstration of Locke very clearly placing **sensory perception as central**, is thus:

'Let us then suppose the mind to be, as we say, white paper void of all character, without any ideas. How comes it to be furnished? Whence comes it by the vast store which the busy and boundless fancy of man has painted on it with an almost endless variety? Whence has it all the materials of reason and knowledge? To this I answer in one word: **from experience**. In that all our knowledge is founded; and from that it ultimately derives itself. Our observation, employed either about external sensible objects, or about the internal operations of our minds perceived and reflected on by ourselves, is that which supplies our understandings with all the materials of thinking. These two are the fountains of knowledge, from whence all the ideas we have, or can naturally have, spring'

(II.I.2, my emphasis)

Locke incorporates certain metaphysical components into his empiricism. The two most prominent are his insistence on "powers" and on causality. In this sense he strays somewhat outside of the "**no metaphysics**" characteristic given for EE. However, as will be seen, we can somewhat make sense of these more metaphysical claims by understanding that for Locke these fall outside the realm of "knowledge" and instead into the realm of "belief".

Powers are properties that objects have to either change something else or to be changed. The former is an 'active' power (ibid, II.XXI.2) and the latter

[29] Interestingly, Locke's use of "idea" is very likely adopted from Descartes (Rogers, 2007, p15)

a 'passive' power. He gives the example of fire and gold. Fire has the active power to melt gold, and gold has the passive power of being able to be melted (II.XXI.1). Locke says that we do not observe powers themselves, but only observe the changes that they make:

'the power we consider is in reference to the change of perceivable ideas. For we cannot observe any alterations to be made in, or operation upon anything, but by the observable change of its sensible ideas'

(II.XXI.2)

Thus powers are posited as an inference to the best explanation regarding how it is that we see objects change. This seems to go against his empiricist doctrine as set out, where all ideas ultimately derive from experience. This is an example of what Russell criticises him for:

'He enunciates general principles which, as the reader can hardly fail to perceive, are capable of leading to strange consequences; but whenever the strange consequences seem about to appear, Locke blandly refrains from drawing them'.

(Russell, 1961, p586).

Causality is a central concept also for Locke. A cause is 'that which produces any simple or complex idea' (ibid, II.XXVI.1). As is the case for powers, Locke never says that we observe the cause itself — as Hume goes on to show, this is not the case — but the cause is inferred. We see the effect, see what always happens when this effect takes place, and infer the cause. This is another instance where Russell's criticism applies, and Locke's empiricist doctrine seems to be at odds with the consequences he draws. It is in Hume that we see these more absurd consequences become drawn out, who uses essentially the same empiricist doctrine as Locke but takes the consequences to their logical conclusions.

When we introduce Locke's views on knowledge then it seems that he doesn't endorse these metaphysical notions as genuine claims to true knowledge that we know with certainty. For Locke, all knowledge is knowledge of ideas that we have (IV.I.1), and 'knowledge is nothing but the perception of the connection and agreement, or disagreement and repugnancy, of any of our ideas' (ibid). There are four types of knowledge: (i) identity, which is knowledge that e.g. white is white, or that white is not yellow. (ii) Relations, which is knowledge of the relation between any two ideas, e.g. that

2+2 =4, or that a square has more sides than a triangle. (iii) Co-existence, or necessary connection, which is knowledge that pertains to two or more ideas that necessarily co-exist or necessarily do *not* co-exist with one another, e.g. that a billiard ball always moves when hit. (iv) Real existence. Concerns the actual real existence, e.g. God.

Powers and causality fall under (iii) — co-existence. Locke says that knowledge of this category is 'very narrow' (IV, III, 10) because there is no 'visible necessary connection' (ibid) between the two ideas. It is not necessary that an effect follows a cause, and it is not necessary that the sun has the power to heat up an object or for the object to be heated. Locke goes so far to say that 'it is impossible we should know which have a necessary union or inconsistency one with another' (ibid, 11). Writing explicitly on powers, Locke says:

'as to the powers of substances to change the sensible qualities of other bodies... I doubt as to these, whether our knowledge reaches much further than our experience'

(ibid, 16).

'I am apt to doubt that, how far soever human industry may advance useful and experimental philosophy in physical things, scientifical will still be out of reach; because we want perfect and adequate ideas of those very bodies which are nearest to us, and most under our command'.

(IV, III, 26)

Locke therefore seems to be introducing powers and causality into his ontology in some sense, but then at the same time clarifying that these, alongside other claims about the world, can never be said with any sort of certainty. We can only know them provisionally, or with good probability. This view is further reinforced in Chapter XIV of Book II, where he discusses *judgements*, and places this type of knowledge into this category.

'The faculty which God has given man to supply the want of clear and certain knowledge, in cases where that cannot be had, is judgement: whereby the mind takes its ideas to agree or disagree; or, which is the same, any proposition to be true or false, without perceiving a demonstrative evidence in the proofs.'

(IV, XIV,3)

Via the restriction of knowledge of the world away from claims of certainty we see Locke's scepticism that is characteristic of his time come in. In this sense Locke fulfils the characteristic of EE of **epistemic scepticism**.

Locke endorses what Newman (2007) calls "epistemological individualism", which rejects knowledge transfer from one person to another (Newman, 2007, p316). Newman points to an example Locke gives: an expert mathematician can be said to have knowledge of a proposition of geometry, but if the mathematician tells the other that this is true, despite the fact that this mathematician can be trusted, the other non-expert cannot be said to have knowledge (Locke, IV.XVI.1). Thus Locke is an exemplar of the **individualistic** criteria set out for EE.

Berkeley

The dominant view within philosophy immediately prior to Berkeley, as touched upon prior, was that (i) there exists a division between primary and secondary qualities, and that (ii) primary qualities were in some sense objective and mind-independent whilst secondary qualities subjective and mind-dependent. Berkeley rejects both (i) and (ii).[30]. He rejects that there can be something qualitatively different between primary and secondary qualities, and consequently rejects that one is mind-independent whilst the other is mind-dependent. Berkeley thus also breaks with the current ontological dualism that was dominant in his time of positing both ideas and matter, and argues that *only* ideas exist. To exist is to be perceived.[31] Berkeley's idealism is not the more conservative view that what can potentially be perceived is what exists, but the more radical view that what exists is what is currently being perceived. He partially gets around this, though, by appealing to God. He says that since God is continually perceiving everything, then we can justify reality in this manner. Importantly, Berkeley is not saying that the things around us don't exist, or that reality doesn't exist. He very much thinks they do. Although all that exists are ideas, the ideas are real in some important sense and should be separated from illusions and mental products.

[30]The rejection of the distinction between primary and secondary qualities is not new to Berkeley, though. Malebranche argues that the argument from relativity of perception was 'just as applicable to primary qualities such as extension and motion' (Woolhouse, 1988, p112) And Bayle questions why the philosophers who reject secondary qualities on sceptical grounds do not go further than just secondary qualities (ibid).

[31]'[f]or as to what is said of the absolute existence of unthinking things without any relation to their being perceived, that seems perfectly unintelligible. Their *esse* is *percipi*'.(Berkeley, 1996, p25)

In his philosophy, Berkeley takes himself to be both rejecting scepticism and embracing common-sense attitudes.[32] This seemingly paradoxical view, given Berkeley's apparently radical conclusion of subjective idealism, can only really make sense if we assume that the two options that we have are his or the orthodox understanding.

The usual combination of the orthodox dualism of materialism and immaterialism apparently led to scepticism because of the following process of reasoning. This dualism supports a difference between things and ideas whereby the material world of things really does exist out there, and then much of what we experience of the material world — i.e. secondary qualities — is just our appearances of those things. So all we ever experience are ideas. But why, then, should we be confident of extrapolating beyond this into the belief that these things really exist beyond what we perceive of them? Locke, who seems to be who Berkeley is responding to most directly, doesn't give any sort of comprehensive answer. And Berkeley believes that this position simply collapses to external-world scepticism. Thus Berkeley's solution is something like: if we all ever experience are ideas, then we should believe that ideas are all that exist.

This last part is a good expression of his empiricism. It focuses heavily on the **priority of sensory perception**, on the centrality of sense-perception as experience, it doesn't want to invoke unobservable entities/structures (**no metaphysics**), and has a general feel of conservativism with regards to not wanting to extrapolate beyond what is necessary. In addition to these points, it is clear that Berkeley's empiricism is **theory-led**, **individualistic**, and is somewhat **sceptical** (despite his denial of being sceptical). His whole method of getting to this conclusion is via abstract contemplation and reflection, and not on practical investigation; it is done through contemplating on what the individual can know; and despite Berkeley's insistence to the contrary, it is sceptical insofar as the conclusion ends up doubting that there really is anything outside of our experience.

Hume

According to Weintraub (2007), the received view in historical research surrounding Hume's relation to Locke is that Hume is a more rigorous and

[32]Through the character Philonous, who is the one advocating his philosophy in his *Three Dialogues*, he has both characters agree that the philosophical position which 'shall appear most agreeable to common sense, and remote from scepticism' (Berkeley, 1996, p109) is true.

consistent empiricist than Locke, and that Hume takes Locke's conception of idea formation and draws out the logical consequences that Locke was too afraid to draw (ibid, p481-482).[33]

Regardless of whether or not Hume is the most consistent empiricist of this time, it is certainly Hume who, at least up until this point of history, is the most influential with respect to the EE in philosophy of science that we see today. Iconic 20th century philosophers that have been explicitly influenced by Hume include the logical positivists/logical empiricists, Reichenbach, Salmon, and van Fraassen, to name just a few.

The three components of Hume's philosophy that are most important to consider this influence are: (1) his problem of induction; (2) his rejection of metaphysics. (2) can be split into (2a) and (2b): (2a) is a rejection of what Kant would call "dogmatic" metaphysics that pertains to metaphysics far outside the realms of human experience. Questions such as "how did the universe begin?", "what is the true nature of Being?", and "what are the fundamental components of reality", amongst others. (2b) is a rejection of a far more conservative metaphysics, which empiricists of a sceptical vein prior to Hume had not considered rejecting. These concepts include causality and the laws of nature. The metaphysics involved in (2b) are the sort of metaphysics that ME often holds on to, and so this marks an especially important divide between the two camps.

To understand Hume's empiricism it's first important to understand his theory of idea-formation in the mind. According to him, there exists an important distinction between *impressions* and *ideas*. The initial perceptions, sensations, passions, and emotions that we feel are *impressions*, and the memories, or 'faint images' (ibid) of these impressions are ideas. Ideas can be further divided into *simple ideas* and *complex ideas*, with the usage being essentially the same as that of Locke's as seen above. The simple ideas are the most basic ideas, and consist of, say, the memory of the impression of the apple, or the memory of the taste of sweetness from biting it, or the colour green associated with it. Then the complex ideas are 2 or more simple ideas combined together in various modes of abstraction. A purple tree is a complex idea that emerges from the simple idea of "purple" and a simple idea of "tree", merged together.[34]

[33] Bennett (1971), Dicker (1998), O'Connor (1952), Russell (1961) all advocate this view in some way. Weintraub (2007) argues against this view.

[34] See (Hume, Enquiry, section 2) for where he lays this out

Hume's copy principle is at the heart of his empiricism:

'all our simple ideas in their first appearance are deriv'd from simple impressions, which are correspondent to them, and which they exactly represent'

(Hume, 2007a, p9)

Thus, if we are to be able to claim to have knowledge of something, then we must be able to trace it to an initial impression of some sort. *If we cannot find an impression that corresponds to an idea, then we cannot claim to have knowledge of it.* The copy principle is the criterion that leads to his rejection of metaphysics and is also a sort-of proto verification principle that we see in the logical positivists. From this it's easy to see how (2) emerges — his rejection of metaphysics. It's easier to see how (2a), a rejection of a more speculative metaphysics, emerges. For example: Descartes' *a priori* principle that the cause must be greater than or equal to the effect is straightforwardly rejected because there's no impression that this idea exists in nature.[35]

The rejection of a far more conservative metaphysics is also entailed by Hume's theory. For instance, the idea that an effect necessarily follows from its cause has no origin in a singular impression, either external or internal :

'When we look about us towards external objects, and consider the operation of causes, we are never able, in a single instance, to discover any power or necessary connexion; any quality, which binds the effect to the cause'.

(Hume, 2007b, p46).

It should be noted that Hume's critique of causality here only poses a problem for EE, and not ME, who do not generally hold sceptical attitudes towards knowledge of relations such as causality. EE, which holds on to the notion of experience basically as sensory-perception, must reject knowledge of relations such as causality from their philosophy for they claim to have knowledge only of what they can experience, and Hume makes clear that this is not the case. ME, though, can easily continue to posit knowledge or existence of causality, since advocates of ME largely don't hold to this conception of experience as limited to sensory experience. We see this made most explicit in James' "radical empiricism" of the next chapter.

Next, and arguably Hume's greatest contribution, is the problem of induction. First, a very brief overview of Hume's "fork". All statements of

[35]This is presented in Descartes' trademark argument in his third meditation

knowledge correspond to either matters of fact, or relations of ideas. In more current usage, following Kant, these are essentially synonymous with synthetic and analytic statements, respectively. Relations of ideas are statements whereby for the statement to be otherwise would be a contradiction (e.g. mathematical truths and tautologies), and matters of fact are the negation of this — for the statement to be otherwise would *not* be a contradiction. Then, relations of ideas do not need experience (a posteriori reasoning) to be verified and can thus be known a priori, but matters of fact can only be verified a posteriori.

Although the overall structure will constantly remain the same, there are different ways of formulating the problem of induction where the difference comes down to order and emphasis. But the following is the way that I believe to be strongest and clearest:

(1) All reasoning concerning matters of fact are founded on the relation of cause and effect. (Hume, 2007b, p19).

(2) Moving from cause to effect requires an inference that nature is uniform in some sense (call this the uniformity principle (UP)).[36] Thus, inferences are founded on the UP, and cause and effect is founded on both inference and the UP.

(3) We need an epistemic justification for the UP.

(4) We cannot justify the UP by a priori reasoning since a priori reasoning concerns truths/statements that could not be otherwise, whereas matters of facts require a posteriori reasoning (from Hume's fork).[37]

(5) And we cannot justify the UP by a posteriori reasoning because this is circular. For any type of a posteriori reasoning uses cause and effect, which in turn uses induction and presupposes the UP. So we are justifying the UP by appealing to something which presupposes the UP and thus presupposes what it is trying to justify.

(6) Therefore the UP cannot be epistemically justified and can't be known through reason.

(7) The UP is a foundational assumption to induction, and therefore if the

[36] 'all our experimental conclusions proceed upon the supposition, that the future will be conformable to the past' (2007b, p25-26).

[37] Section IV, part 1 of the *enquiry* boils down to this

UP can't be know through reason then induction can't be known through reason.

The subtleties of Hume's problem of induction is formulated differently depending on what account you read, but it can be seen to have three key components.

(i) *The Uniformity of Nature Principle Component*: To make an inference into the future, or from particular to general, we require the assumption that things will basically remain the same to a sufficient enough degree for the inferences to hold.

(ii) *The Sceptical Component*: When we make inferences into the future, there is no necessity or certainty that will hold the Uniformity Principle.

(iv) *The Epistemic Justification of the UP*: We need a justification for the UP, since it presupposes all inferences, but we can't justify it through a priori means and nor can we justify it through a posteriori means.

(v) Conclusion: The UP therefore cannot be rationally justified and since induction rests on the UP, induction can also not be rationally justified.

Concluding Remarks

To recap: this chapter does two things. First, it sets out what empiricism is, and sets out the distinction between EE and ME via a set of criteria. Second, it traces the history of both of these versions of empiricism, starting in Ancient times and ending at the end of the Early Modern period with Hume. The purpose of both of these tasks is to partially achieve the first aim of this project: to demonstrate that there exists an alternative version of empiricism (ME) that has existed both past and present, and that it has been heavily neglected. The latter part of this aim has not been achieved yet. This is done in chapter 3.

The purpose of this and the subsequent chapter's historical focus is to shed light on the current situation in philosophy of science, with respect to its attitudes towards empiricism, to gain a better appreciation of the history of empiricism with a special focus on the more neglected version of empiricism, ME. The novel contribution here is thus in this respect — to reinterpret, in some sense, specific empiricist philosophers and situate them in respect to both EE and ME in current philosophy of science.

Chapter 2

Empiricism After Hume

Introduction

This chapter traces the history of empiricism from where the last chapter left off, focusing on post-Humean empiricists up until the logical positivists/logical empiricists. As per the last section of the previous chapter, each philosopher is explicated with reference to the characteristics of EE or ME that is more appropriate. The philosophers discussed in the Early Modern period tend to fit more determinately into either EE or ME. With philosophers in this chapter, the distinctions are at times more blurred for EE, whilst remaining somewhat more consistent with the categorisation of ME. It is hopefully clear that this is not a problem for this thesis. The version of ME remains more consistent a position than does EE, where the latter becomes what empiricism is conflated to despite there being more historical consistency with the position of ME. The ways in which those who are best situated in the EE tradition as opposed to the ME tradition — Mach and Mill — stray from EE is with respect to holding more characteristics that are suitable to ME than Early Modern EE.

There are some philosophers that are surely empiricists in some sense from this period that have been omitted here. Given the constraints on the size of this chapter, I have chosen what I take to be the empiricists that are most significant for current philosophy of science. The main figure that is omitted is Quine. Quine presents an empiricist philosophy of some sort, and is undoubtedly incredibly influential in both philosophy and philosophy of sciene. But the influence is *not* in virtue of his empiricism, at least within current philosophy of science. When philosophers of science reference Quine, it is primarily for his naturalistic approaches to traditional philosophical subjects, or the Duhem-Quine thesis, or his critique of logical positivism in the iconic paper "two dogmas of empiricism". The latter in fact frames Quine as arguing against empiricism in an important way. Thus, to whatever extent he is an empiricist, it is not this empiricism that has great influence within philosophy of science.

2.1 19th Century Empiricism

In addition to the thinkers covered here, there are several other who are heavily influenced by the more pyschological/philosophy of mind aspects of Locke's philosophy, i.e. his conception of how we form ideas and the mind as a blank slate. This formed the school of Associationism, of which prominent members included David Hartley (1705 - 1757), Joseph Priestley (1733-1804), James Mill (1773-1836). See Mandelbaum (2020) for more details on this.

2.1.1 Epistemic Empiricism

Significantly, the figures that I describe here cannot be said to be staying true to EE in the same way that figures in the Early Modern tradition do. There are certain places where they have characteristics that fit more into ME, and places where the characteristics of EE listed simply do not exist in their empiricism. I have nonetheless chosen to place them into this version since they have more in common with EE than they do ME. It is important to keep in mind this initial point for the purpose of the broader project — despite philosophers of science conceptualising empiricism as being limited to EE, in the 19th century no philosophers really hold anything strictly akin to it.

Comte

Comte (1798-1857) was the founder of positivism, and although not really an empiricist himself, he was influential on other empiricists of his time. It is consequently worth briefly discusing his important ideas. The text where the positivist philosophy is most extensively set out is his six-volume *Cours* *de philosophie positive.*[1] This was first given as a series of lectures beginning in 1826, and then subsequently published in stages between 1830 and 1842. There are parts of this philosophy that are thoroughly un-empiricist. For instance, a metaphysics of historical development, similar in structure to a Hegelian or Marxist philosophy of history, is introduced via a 'fundamental law' (Comte, 1988, p1) that governs historical development. This law moves humanity through three stages: first, the theological stage, then the metaphysical stage, and finally the scientific/positive stage.

But Comte's conception of what this final stage of the human mind/knowledge looks like resembles an empiricist outlook and incorporates elements of both EE and ME. For example, he directly references Francis Bacon:

'All competent thinkers agree with Bacon that there can be no real knowledge except that which rests upon observed facts'

(ibid, p4).

He argues not only for the existence of laws within science under this positivist stage of human development, but that 'the fundamental character of the positive philosophy is to consider all phenomena as subject to invariable natural laws' (ibid, p8). However, he is very clear on his desire to rid science of metaphysics, and of metaphysical questions, and asserts that explanation should not be required past a certain point. Giving the example of phenomena in the universe, why they move how they do is explained by Newton's law of gravitation (ibid). And explanations beyond that that start to become metaphysical or theological, are not part of this positivist philosophy. Questions about what things like "attraction" or "weight" actually *are*, in and of themselves, are questions that are considered insoluble by the positivists (ibid, p9). Overall then, there is clearly a desire to shift away from overly speculative metaphysics and into scientific understandings of phenomena; but there is just as clearly much metaphysics left over in this conception.

Mill

An important distinction is at the heart of Mill's empiricisms between *verbal* propositions and *real* propositions. These broadly correspond to the divisions made in Hume's fork between "relations of ideas" and "matters of fact", and to Kant's division that was based on this between analytic and

[1] Course of positive philosophy.

synthetic, respectively (Mill, CW, VII, p115). Verbal propositions give us no new information about the world but are 'unfolding the whole or some part of the meaning of the name' (Mill, CW, VII, 113), whereas real propositions give us more information than what is contained in the proposition, and 'add to our knowledge... not already involved in the names employed' (ibid, 115-116). Importantly for Mill, *verbal propositions should not be considered truths*. Mill thus breaks from the others in the EE tradition from the earlier *Early Modern* period — such as Gassendi, Locke, Berkeley, and Hume — who all considered analytic statements, although not using the term analytic, to be necessary truths. Mill differs here also from future thinkers in the EE tradition that will be seen, including the logical positivists in the 20th century. Equally importantly, there are certain types of propositions that Mill considers to be real/synthetic that go against the orthodoxy. Most famously are mathematical and geometrical truths (ibid, p224-262). This, in some sense, brings some orthodoxy back to Mill's empiricism insofar as mathematical and geometrical propositions are truths in the same way that others in the EE tradition have considered them to be so; but it leaves many other analytic/verbal statements that are tautologous as not capable of being true or false.

Real propositions, unlike verbal propositions, have the potential to be either true or false. The *true* real propositions can be known either (i) through direct, immediate experience — e.g. through immediate sensory impressions that we have — or (ii) through inference. The logic that Mill is interested in developing in *A System of Logic* is concerned with the latter. Importantly, sensory experience is at the heart of this, thus displaying the EE characteristic of the **centrality of sense perception**.

The inferences that are the focus here are ampliative inferences going *from* the immediate sensory impressions that we have *to* more complex and less obvious truths. Mill is thus a foundationalist — the truths that are known directly are the foundational truths from which other inferences are made. A clear passage where Mill sets this out is below:

'Truths are known to us in two ways: some are known directly, and of themselves; some through the medium of other truths. The former are the subject of Intuition, or Consciousness; the latter of Inference. The truths known by intuition are the original premises from which all others are inferred. Our assent to the conclusion being grounded on the truth of the premises, we could never arrive at any knowledge by reasoning, unless something could be known antecedently to all reasoning'

(Mill, CW, VII, p6-7)

In the same vein as previous thinkers within the EE version that have been discussed, but made most explicit in Hume via Hume's fork, real/synthetic propositions are known only *a posteriori*, or through experience:

'Of nature, or anything whatever external to ourselves, we know... nothing, except the facts which present themselves to our senses, and such other facts as may, by analogy, be inferred from these. There is no knowledge a priori; no truths cognizable by the mind's inward light, and grounded on intuitive evidence. Sensation, and the mind's consciousness of its own acts, are not only the exclusive sources, but the sole materials of our knowledge'

(Mill, CW, X, p125)

Regarding characteristic (3) — **no metaphysics** — it is hard to make a clear judgement on this. Mill does sometimes endorse what would be considered by today's standard as metaphysics. He believed that we can genuinely come to know causes through four specific methods of induction (Macleod 2016)(Cobb, 2017, p240-241). As science develops, our methods of induction become more complex and sophisticated and thus allow us to find more causes and laws of nature (Macleod, 2016). And Mill did believe that science reveals how things genuinely are (Macleod, 2016)(Mill, CW, VII, p295, p651). Macleod points to a few quotations from Mill: 'Kinds have a real existence in nature' (ibid, p122), and that scientific analysis of nature uncovers a 'radical distinction in the things themselves' (ibid, p123).

But Mill conceives of laws simply as regularities: 'The expression, Laws of Nature, *means* nothing but the uniformities which exist among natural phenomena' (Mill, CW, VII, p318). He thus seems to take a Humean view with regards to natural necessities and laws. Further, the way in which one would infer from sensory impressions to the reality of some unobserved phenomena like a cause is seemingly through an inference to the best explanation. Mill does not endorse this line of reasoning, though. Skorupski (1994, p197-202) and Macleod (2016) point out that Mill rejects what is now called inference to the best explanation, with his reasoning closely resembling something akin to the problem of underdetermination:

'[A hypothesis] is not to be received probably true because it accounts for all the known phenomena; since this is a condition sometimes fulfilled tolerably well by two conflicting hypotheses'

(Mill, CW, VII, p500).

And it seems at points that Mill embraces an entirely sensationalist/phenomenalist metaphysical picture. For instance:

'Of the outward world, we know and can know absolutely nothing, except the sensations which we experience from it'

(Mill, CW, VII, p62)

Despite writing almost a century after the publication of Hume's problem of induction, Mill doesn't recognize or address the problem of induction (Skorupski, 1994, p100). Randall (1965) says that it is doubtful that Mill ever read Hume, but was familiar with him as a philosopher. The uniformity principle that Hume said could not hold up as a rational justification for ampliative inferences due to the circularity of doing so is exactly what Mill invokes to justify induction.[2] And so *A System of Logic* is not an attempt to provide a deeper justification of induction, but takes this as a given and sets the task as being to 'supply a set of criteria or tests which can be used to distinguish good from bad inductive inferences' (Godden, 2014, p59).

To make explicit what was only implicit: Mill's empiricism is **individualistic** — the way he considers how one can obtain knowledge is through the framework of how an individual can do so. The idea of gaining knowledge for Mill is through the individual's sensory experience or through inferences that the individual can make from these. The way at which Mill arrives at his philosophy is **theory-led** — it is abstract contemplation on the matters at hand.

Mach

Mach's empiricism is firmly situated within an evolutionary framework, which was influenced not just by Darwin but also by Herring and Hackel (Pojman, 2019). This is evident in at least two elements: his external (science as viewed from the outside) and internal (the internal processes within science) characterisations of science. Externally, Mach thinks that science develops organically as humans develop.[3] And there is then a feedback loop

[2]'that the course of nature is uniform; that the universe is governed by general laws [...] [is] our warrant for all inferences from experience .... [and] the fundamental principle, or general axiom, of induction' (Mill, CW, VII, p306-307).

[3]'Scientific thought arises out of popular thought, and so completes the continuous series of biological development that begins with the first simple manifestations of life'

between scientific development and the development of the evolutionary process of humans; science becomes not just *a*, but *the* most important factor in the ongoing evolutionary development of humans.[4]

As Pojman points out, for Mach science 'is both an outcome of this process (it has biological roots), and also now is part of this process' (Pojman, 2019). Pojman also says that Mach's situating of science within this framework contributes towards his approach to science as something that should be as economical as possible (ibid). In this sense — placing the stress on science as a collective enterprise that gathers knowledge through and for the development of a community — Mach's focus is ***not* individualistic**, and thus in this sense fits more into ME.

Mach's empiricism satisfies the **no metaphysics** characteristic of EE, and this is arrived at partially through his conceiving of science as aiming for the economy of thought. But this desire to not want to introduce any metaphysical claims is not due to a general scepticism — and thus fails to satisfy the EE characterisation of **epistemic scepticism** — but is brought about instead by the positive findings of science, namely through the emerging field of psychology:

'To whatever extent he was a phenomenalist, he was a scientific phenomenalist. His ideas were not derived from scepticism but through application of the results of psychology and evolutionary theory.'

(Pojman, 2019).

Many of Mach's ideas were **arrived at through scientific practice via experiments**, and *not* through theoretical contemplation. Mach bands, which were foundational to positive formulations of his empiricism, were arrived at through an experiment (Blackmore, 1972, p49). Similarly, his work to relate hearing with ear structure and sensing motion were examined through experimentation (ibid, p52). Hiebert writes on this:

'Mach, it seems, was downright uncomfortable, intellectually, about general theoretical syntheses. The more general, the more uncomfortable he became. He was at ease with facts (Tatsachen), thoughts (Gedanken), the adaptation

(Mach, 1976, p1).

[4]See: 'today we can hardly doubt that it has developed into the factor that is biologically and culturally the most beneficial. Science has taken over the task of replacing tentative and unconscious adaptation by a faster variety that is fully conscious and methodical' (Mach, 1976, p361).

of thoughts to facts, that is, observation (Beobachtung), and the adaptation of thoughts to other thoughts, that is, theories (Theorien). More than that was unjustifiable'

(Hiebert, 1976, Pxxii)

Mach certainly **holds to the centrality of sense perception**, and thus satisfies this criterion of EE. Whilst traditionally Mach has been read straightforwardly as a phenomenalist, Preston (2021) points out that there have been others who read Mach as not being so, and himself advocates such a non-phenomenalist reading.[5] In the latter camp the most notable are Russell (1956) and Banks (2014). But regardless of where one falls in this interpretational divide, that Mach places sensory experience front and centre of his empiricism cannot be doubted. It may seem confusing that Mach was both an experimentalist and yet still places sensory experience as central; *prima facie* it seems like the two are in conflict. Experiment seems to allow us to probe deeper into the world, and come to be aware of phenomena that are not apparent to sensory experience. The two views become compatible, with Mach, when we take into account how Mach conceived of experiments. He claims that experimentation is simply a form of human experience, one that involves active observations as opposed to passive observations:

'Man collects experiences by observing changes in his surroundings. However, the most interesting and instructive changes from him are those that he can influence through his own intervention and deliberate movements... That is what makes experiments so valuable'

(Mach, 1976, p134).

Mach, in Knowledge and Error, conceives of experiments as requiring a thought experiments as a necessary precondition (1976, 136), and that the experimenter 'must have planned the arrangement in his head before translating it into fact' (ibid). This is at odds with those in the ME tradition that were discussed in the previous chapter, whose vision of experiment is instead "experiment-as-discovery"; that is to say, that experiment is conceived of as being the way in which we investigate nature, and can learn genuinely novel and real things about the world.

[5] Amongst those who read Mach as a phenomenalist, Preston (2021) points to Avenarius, Kleinpeter, Carnap, Frank, Haller, Lenin, Husserl, Popper, Holton, Blackmore.

2.1.2 Methodological Empiricism

The focus of this section is on four thinkers in the 19th century. Three of them were closely connected — William Whewell, Charles Babbage, and John Herschel. All were heavily focused on experiment and took great inspiration from the work of Francis Bacon. Their early meetings and philosophical ideas had their roots in a philosophical breakfast club that they organised during their student years at Cambridge between 1812-1813 (Synder, 2011), and the topics that they would have weekly discussions over would generally be selected passages from Francis Bacon's work (ibid, p37). All three were motivated by the idea that the scientific method required a restructuring from how it currently existed in their time, and that it should look far more like what Bacon described (ibid, p42-43).

Whewell

At the heart of Whewell's (1794-1866) philosophy of science is an epistemology that is somewhat Kantian in nature, but with important and significant differences. There are several antitheses that Whewell introduces, and shows that whilst they are prima facie distinct and dichotomous, they are actually far more symbiotic than they appear (2014a, p1-79). These include sensations and ideas, theories and facts, and within the category of ideas, "Fundamental ideas" and "Ideal Conceptions".

Essentially, Whewell seemingly wants to strike a middle ground between two positions: against the more crudely formulated EE of the Early Modern period, and the more speculative rationalism. In this he follows Francis Bacon's desire to strike a middle ground between the empiric ants and the spider-like rationalists that was discussed in (1.4.1). Those who advocate EE want to frame everything around sensations, whereby all of our ideas are simply us remembering and modifying the sense-impressions that we have had in the past; the rationalists want to frame everything around ideas, whereby reality is just ideas. For Whewell it is a combination of these. Ideas and sensations both form intergral parts of the mind and how we come to gain genuine knowledge of the world, and exist in a relationship whereby they can never really be instantiated one without the other, yet their opposition form an integral role 'of the most essential importance' (Whewell, 2014a, p29) to philosophy.

We gain knowledge of the world in the following manner: sensations come to us from the real external world, and ideas give sensations their particular character in our minds (ibid, p25-26). When we start to grasp several or more of these brute sensations and conceptualise them to see connections and relations between them, we develop *theory*. For example, theories that the tide moves in such a way because of the pull of the moon. Once theories become more established and confirmed, they themselves become facts which the more enlightened individuals will become aware of. It is a fact, for example, that the gravity of the moon controls the tides, in the aforementioned example, and that the earth moves around the sun, despite initially starting off as only a theory (ibid, p20-22). In the process of moving from facts to theories, we add in various conceptual additions to the theory that are *created by our minds* and are therefore *ideas*. These include concepts such as *causality*, *space*, *unity*, and Whewell calls these "Fundamental Ideas" (ibid, p25-35). Fundamental Ideas are *a priori*, and clearly Kantian. We impose these Ideas onto the world and we require them to be able to make sense of anything.

There is clearly a Kantian dimension to Whewell's thought, but other scholars have made clear that the epistemology differs from Kant's in important ways.[6] Most importantly here is that the Fundamental Ideas of Whewell's philosophy *really do* tell us about, and tell us accurately, about objective features of the mind-independent world, contra Kant's anti-realism.

In dismissing knowledge or beliefs in being based purely around sensations that we get from our senses, Whewell thus **de-centralises sensory perception** as the primary part of both coming to gain knowledge or investigate the world. It is clearly an important and necessary component, but is not sufficient.[7]

An absolutely vital point in Whewell's philosophy of science is his Baconian style inductivism, which is central to this process of gaining knowledge. When we move from fact to theory, we use induction, and in using induction we apply Fundamental Ideas and other Ideal Conceptions — which are ideas that are derived from Fundamental Ideas (ibid, p37-40) — to the facts in order

[6]Synder (2017) points out that Whewell didn't follow Kant in distinguishing between precepts such as space and time and the categories, or forms of thought. Whewell included also "Fundamental Ideas" which don't function as conditions of experience but as conditions for having knowledge within their respective sciences (ibid). And unlike Kant, Whewell didn't intend to give an exhaustive list of "Fundamental Ideas", believing that more will develop in the emergence of science (ibid).

[7]Whewell writes: 'The combination of the two, of ideas and experience, is, as we shall see, necessary, in order to give us any knowledge of the external world, any insight into the laws of nature' (Whewell, 2014a, p33).

to both make sense of the world and to expand our knowledge from more simple, brute empirical facts, into coherent world-pictures. Using induction, we move from fact to theory and then back to fact via the above process, and as science develops we make further generalizations and introduce new theories, and these theories once again become facts once they become more established. And so on and so forth, and this *is* science.

Like Bacon, Whewell consistently talks about knowing Nature, which asserts a position of scientific realism of some variety and thus displays **willingness to endorse some metaphysics**. Whewell was certainly not a global sceptic of any sorts. He believed that there really do exist certainties and truths which we can know, and that the best way to find these is through science (Whewell, 2014a, p4). Much of his work is concerned precisely with the ways in which we can come to access these truths, largely via exploring the scientific method and processes of science. Whewell also refers throughout his (2014a) and (2014b) to "laws of nature" both as things that do exist and things that can be known.

Whewell adopts elements of **philosophy-first epistemology**, which ME rejects. As can be seen above, there is clearly much background theorising that takes place in his epistemology that is not deferred to scientific practice or even science in general. He thus does not satisfy this criterion of ME.

Whewell does adhere to the **empirical investigation** and **practice-led** characteristics of ME, and this is displayed through his scientific career. The science he did was **practice-led**, and was constantly based around experiments, gathering data and working out exactly what we can know, and held a generally empirically-led and practice-led approach. One instance includes his mapping of the tides. Whewell picked out teams of observers from the public and scientific community, and instructed them to make frequent and precise measurements of tide levels in various places around the world (Snyder, 2011, p174).[8] The data was then taken, analysed, and plotted into tables to show various correlational patterns betwen variables such as the wind, weather, and their position in the world (ibid, p177). This allowed for predictions and for causal statements to be drawn up.

Whilst the practice-led component is true of his activities as a scientist, there are certain passages in his philosophy that may throw this into question when considering Whewell overall. For instance, he writes of studying nature and

[8] As Synder documents, nine countries were involve in this — the United States, France, Spain, Portugal, Belgium, Denmark, Norway, the Netherlands, the UK, and Ireland

knowledge for its own sake before having to worry about practical concerns surrounding this (Whewell, 2014a, pxiv), in contrast to Francis Bacon's desire to focus on knowledge for its practical rewards. But this is overshadowed by a greater, overarching theme of his entire philosophical work — that he is constantly looking to the sciences themselves, in practice, to inform his philosophy. The book he writes prior to his philosophical treatise (philosophy of the inductive sciences) is his *History of the Inductive Sciences*, which he draws upon heavily in his claims; and many chapters of both volumes of the *philosophy of the inductive sciences* are direct examinations of specific fields of the science of his day. He was also critical of Herschel for not placing enough focus on actual empirical case studies in the history of science (Synder, 2011, p251).

Herschel

John Herschel (1792-1871) was the son of William Herschel, the astronomer who discovered what went on to be called Uranus. He was thus brought up in and immersed in a scientific background. After trying and failing to gain an academic post after Cambridge, and trying but disliking private tutoring, he returned home to work with his Father on his astronomical studies.

Herschel was heavily immersed in experiment and practically-based science, with theory always being of second concern to him, in accordance with a **practice-led** philosophy. When younger, Herschel began experimental life by trying to recreate the experiments of others, before moving on to the experimental investigation of phenomena such as the crystalline structure of bicarbonate of potash and general studies of the optical properties of crystals (Snyder, 2011, p51-52). Amongst one instance of the latter, he specifically examined the iridescent inner lining of mollusk shells, which were built up by layers of a calcium carbonate and proposed an explanation for this as being due to interference in light waves (based on the work of Thomas young) (ibid). Importantly, when proposing these explanations and proposing to try to discover what the world is like, it was empirical investigation that Herschel believed is the best way to proceed.

The rest of Herschel's career puts into practice this view that **empirical investigation** is the best way to investigate the world. After the experiments just described, he went to work with his father, making astronomical observations with the state-of-the-art telescope. Later on in his life, after the writing of his magnum opus that will be discussed below, he spent four years in what was then called the Cape Colony to map the stars in the southern hemisphere (Synder, 2011, p158-170). He also made important scientific contributions to the field of photography.

Herschel's main work where he fleshes out his philosophical views are in his *Preliminary Discourse on the Study of Natural Philosophy.* We can see the Baconian influence straight away: the book has a quote from Bacon's *Novum Organum* in the title pages, and a picture of a bust of Bacon here too. In the text, Herschel argues that experience is at the foundation of the natural sciences (Herschel, 2009, p76), and divides experience as functioning in one of two ways when doing natural science: *observation* and *experiment.* Observation is passive, he says. It is noticing facts as they occur and without any interference (ibid). Experiment is active — it is the manipulation of parts of nature that we can control, 'putting in action causes and agents over which we have control, and purposely varying their combinations, and noticing what effects take place' (ibid). Importantly though they are not different in kind, but simply varying degrees of the same kind (ibid, p77) — indeed, he says that the terms "passive observation" (for observation) and "active observation" (for experiment) may better describe the two terms. And it is experience, in either of these forms, that should be the *only* reference to which science should study the world (ibid, p80).[9] An important precursor to natural science, which is a prerequisite in order to be able to use experience properly, is to rid ourselves (qua inquirers) of certain prejudices.[10] One of these prejudices is the prejudices of the senses — of naked sensory experience (ibid, p81-85). It is thus not naked sensory experience that Herschel appeals to as the experience that forms the foundation of the natural sciences, but the more carefully considered form that we can see via *observation* and *experiment.*

The other prejudice that he insists we must rid ourselves of before beginning scientific investigation is "prejudices of opinion". These are prejudices that arise from either 'the assertion of others, from our own superficial views, or from vulgar observation' (ibid, p80) and which have formed to dogmas in our mind. These and our naked, unanalysed sensory experience, we should be critical of. Thus we see Herschel's **mild scepticism** — he holds something of a local scepticism in respect to these topics, but is certainly not a global sceptic about knowledge and scepticism does not drive his philosophy.

[9]'To experience we refer, as the only ground of all physical enquiry' (Herschel, 2009, p80)'.

[10]This process is surely influenced by and akin to Francis Bacon's insistence that we must rid ourselves of the "Four Idols", seen in the previous chapter. We see Whewell also insist on a similar process (see above).

Herschel **allows for metaphysics**, in some form. He is by today's standards a scientific realist, talking frequently in his (2009) about knowing Nature in the sense that Francis Bacon means, and also frequently talks about causality and laws of nature as being both real and knowable through science. Indeed, he writes that cause and effect, and laws, are 'the ultimate relations we contemplate' (ibid, p76) in natural science.

Jones

Richard Jones (1790-1855) is known primarily for his work on economics, and in particular his critique of Ricardo's economic views. At the crux of this is an empiricist economics that fits specifically into the version of ME. Ricardian economics was based around a few apparently universal axioms which were seemingly at odds with the empirical facts, and thus employed a deductive method from which the economic system is derived from these facts (Rashid, 1979, p160). Jones' empiricist critique of Ricardo's system was threefold: (i) that Ricardo had claimed a false universality for the theory which is not reflected empirically, (ii) that Ricardo theorized upon categories that were unobservable in practice, (3) made historically inaccurate claims that had been empirically falsified (ibid). Jones' criticism was based around detailed studies of empirical data from both past and present (Rashid, 1979), with which he found Ricardo's system heavily at odds with what he had found.

The empiricism displayed by Jones is displayed consciously. He writes:

'We must get comprehensive views of facts, that we may arrive at principles which are truly comprehensive. If we take a different method, if we snatch at general principles, and content ourselves with confined observations, two things will happen to us. First, what we call general principles will often be found to have no generality, [...] and, secondly, we shall miss a great mass of useful knowledge, which those who advance to principles by a comprehensive examination of facts, necessarily meet with on their road'

(Jones, 1859, p569).

And in the preface to Jones' collected works, Whewell writes that this "inductive disposition" that Jones displays belonged to him from an 'early period' (Whewell, 1859, pxix), and that Bacon's Novum Organon was 'one of their favourite subjects of discussion' (ibid).

It is hopefully clear just how Jones' stresses the need for the primacy of

empirical investigation, and also advocates for and employs a **practice-led** approach. The economic criticisms that he has of Ricardo demonstrate this, as do the above quotation. The method of axiomatic deduction that Ricardo employs must be replaced, he insisted, with an empirical, fact-driven methodology. Jones was primarily concerned with economic methodology, and not with that of epistemology. This thus demonstrates a clear **shift away from philosophy-first epistemology**, since there is no real epistemology to speak of. There is nothing significant in Jones about sensory perception being of particular importance above and beyond other forms of empirical investigation, and thus Jones can be said to **decentralise sensory perception**, in accordance with ME.

William Thomson

William Thomson (1824-1907) was a British polymath — an engineer, physicist and mathematician — who made enormous contributions to science. He had a major role in the development of the second law of thermodynamics, the absolute temperature scale, the analysis of electricity and magnetism, the geophysical determination of the age of the earth, and work in hydrodynamics (Sharlin, 2021)(Chang and Yi, 2005). On a more practical level, he was involved in the laying of the first transatlantic cable. He was knighted and made a lord — Lord Kelvin — of which the temperature scale is named after.

Thomson does not develop anything like a proper epistemology. He thus, in virtue of not having anything that can be called an epistemology, **does not have a philosophy-first epistemology**. Thomson is clearly and directly influenced by Herschel and is clearly following in the Baconian tradition in some sense. He explicitly adopts Herschel's use of the term "experience", defining it as 'our means of becoming acquainted with the material universe and the laws which regulate it' (Thomson and Tait, 1879, p440). He also adopts Herschel's understanding of the difference between observation and experiment. Experiments are active observation, for Thomson. And they are necessary in science because of the fact that in everyday observation we cannot isolate causes due to the complexity of regular experience with respect to the plethora of causes that could be responsible for any given effect (ibid, p443). Thus already we can see Thomson's **willingness to endorse metaphysics** by holding a realist position about causes, of which we know through science.

He follows also in the Baconian tradition with his approach to Natural Histories that was first highlighted in (1.4.1), which is also endorsed by subsequent thinkers in the ME tradition. He writes:

'Observation, classification, and description of phenomena necessarily precede Natural Philosophy in every department of natural science. The earlier stage is, in some branches, commonly called Natural History'

(Thomson and Tait, 1879, pv)

At points he seems to become close to prioritising sensory perception: defining the concept of matter briefly as 'that which can be perceived by the senses' (p219). However, there is good reason that he does not hold strongly to this, and to suppose that it is a badly-phrased remark. He instantly defines it — perceiving this to be synonymous — as 'that which can be acted upon by, or can exert, force' (ibid). Clearly the two are not equal, as force cannot be perceived by sensory perception. In other places he talks of legitimate observations being made which clearly do not rely on sensory perception (Thomson, 1891, p288). He also discusses being able to measure electrical currents (ibid, p443), and treats measurement as the source of greatest epistemic privilege:

'I often say that when you can measure what you are speaking about, and express it in numbers, you know something about it; but when you cannot measured it, when you cannot express it in numbers, your knowledge is of a meagre and unsatisfactory kind'

(Thomson, 1891, p73)

Thus, we can infer from this that he holds no special status for sensory perception, and thus matches the characteristic of **de-centralising sensory perception**.

He has focused greatly on both **empirical investigation** and oriented his philosophical views **towards practice**. There is extensive writing in his (1879) of both experiments and of measurement, including a chapter on various types of measurement that can be performed, with a focus on scientific practice: on actual measurements that scientists make and have made in order to gain results (1879, p457-478). He advocates a methodology, when empirically investigating, of trying many different techniques and styles:

'Endless patience and perseverance in designing and trying different methods for investigation are necessary for the advancement of science: and indeed, in discovery, he is the most likely to succeed who, not allowing himself to be disheartened by the non-success of one form of experiment, judiciously varies his methods, and thus interrogates in every conceivable useful manner the subject of his investigations.'

(Thomson and Tait, 1879, p443)

2.2 Pragmatism

In the pragmatists, we see another stage and version of ME which differs from the original form of it in Bacon, Boyle, and the Early Modern experimentalists, and the 19th century ME of Whewell, Herschel and Jones. With the former two styles, the concern is very much with science, and methodologies of science. With the pragmatists, there is far more of a focus on philosophy itself. The traits are very much the same in that they fit the characteristics that I laid out in chapter 1, but the focus for the pragmatists is *not* on creating a new methodology of science, but on *reconceptualising philosophy*. They drew heavily on and were influenced by results in science and experiments, and were involved in science themselves (e.g. James and Dewey both worked in psychology); but nonetheless much of the work they did was meta-philosophical, reflecting on issues such as critiques of the early-modern EE of Locke and Hume and general EE conceptions of sensory perception, critiquing prior assumptions of modern philosophy such as the mind-body division and the Cartesian model of the subject-object.

One thing that runs common to the essence of pragmatism is the **rejection of a global scepticism**. Aikin and Dabay (2018) characterise the pragmatist response to the sceptic as being essentially a 'redifinitive' (Aikin and Dabay, 2018, p105) programme which consists of two parts: (i) the justification of knowledge, and (ii) the truth-conditions of knowledge (ibid). As the name suggests, the pragmatists redefine the terms which the sceptic has traditionally used with respect to these two parts. The first part consists in the epistemic fallibilism that runs common to pragmatism, whereby beliefs don't require justification that is not open to doubt. All beliefs can be doubted and nonetheless justified. The second part consists in the introduction of the pragmatist theory of truth, which holds a more relativistic account of truth whereby 'truth is a function of how our beliefs serve our interests' (ibid, p106).[11] Thus the pragmatist movement rejects the scepticism of EE.

William James and Radical Empiricism

James introduces "radical empiricism" in *The Meaning of Truth*, and defines it as consisting of (i) a postulate, (ii) a statement of fact, and (iii) a generalized conclusion (James, 1987, p826).

(i) *The Postulate*: 'the only things that shall be debatable among philosophers shall be things definable in terms drawn from experience. [Things of an unexperiencable nature may exist ad libitum, but they form no part of the material for philosophic debate]'. (Ibid).

(ii) *The Statement of Fact*: 'the relations between things, conjuctive as well as disjunctive, are just as much matters of direct particular experience, neither more so nor less, than the things themselves' (ibid).

(iii) *The Generalized Conclusion*: 'the parts of experience hold together from next to next by relations that are themselves part of experience. The directly apprehended universe needs, in short, no extraneous trans-empirical connective support, but possesses in its own right a concatenated or continuous structure' (ibid).

From (i), we see that James intends to do away with philosophical discussion of a *particular type* of metaphysics; namely, one that exists outside of claims that can experienced. This thus seems to fit into the characteristic of no metaphysics of EE. However, we see in (ii) that relations between phenomena are claimed to be experienced in a way that would now typically be characterized as a metaphysical claim. We should therefore classify this position as **allowing for metaphysics** and thus fitting more into ME in this respect. From this fact we also see that James proposes a conception of experience which certainly doesn't fit into the EE category of experience-as-sensory-perception. It explicitly goes against Hume's empiricism (see (James, 2018b, p24-34)), which as was seen in the previous chapters claims that relations such as causation are not experienced, and are thus not strictly knowable in an empiricist framework. In this sense, then, James fits within ME **not holding sensory experience as central** with his account of what experience is.

[11] Importantly, though, the pragmatist theories of truth differed from thinker to thinker. Peirce held a theory of truth that was more objective and did correspond to reality in some significant sense, whereas James held a pluralism about truth (Legg and Hookway, 2021).

James **rejects the individualistic** nature of EE, criticising the atomisation that EE of, for example, Locke and Hume implement. This incorporates the notion of the "self", or the individual subject that I listed as an important characteristic of EE. Through his introduction of the reality of relations and thus as introducing experience as continual and not disjointed, he also sees the removal of problems that have plagued philosophy such as how the subject can come to have knowledge of the object (James, 2018b, p30). The problem boils down, ultimately, to treating the two as entirely separated, which James (and as will be seen, Dewey) take to be wrong. James thus again fits into the methodological empiricist version in this respect.

James holds to the pragmatic method, which is thoroughly **practice-led**. He defines this as:

'The pragmatic method is primarily a method of settling metaphysical disputes that otherwise might be interminable. [...] The pragmatic method in such cases is to try to interpret each notion by tracing its respective practical consequences. What difference would it practically make to any one if this notion rather than that notion were true? If no practical difference whatever can be traced, then the alternatives mean practically the same thing, and all dispute is idle'

(James, 1922, p45).

Thus, the empirical consequences of a statement are taken to be the defining test of the legitimacy of that phenomena. We should not be concerned with abstract, theoretical reasoning; or on what is theoretically the case. We should turn to practice, and to the practical consequences of things. He here also points out that the term 'pragmatism' is derived from the Greek word for action, from which the term "practice" and "practical" arise (ibid, p46).

Dewey

Dewey calls his style of empiricism either 'empirical naturalism' (Dewey, p1a), or 'naturalistic empiricism' (ibid). Dewey certainly holds to the methodological empiricist characteristic of conceptualising experience as something that should be used for **empirical investigation**: chapter 1 of his *Experience and Nature* makes this extremely clear. His empirical method, outlined in this chapter, splits experience into two types. He contrasts 'primary experience' (Dewey, 1958, p3) with a 'secondary' (ibid, p4) type of experience. Primary experience is the immediate, unreflected type of experience that we have constantly; secondary experience arises out of 'continued and regulated reflexive inquiry' (ibid). The products/objects of science and philosophy, he says, are products of the secondary type of experience (ibid). But where science differs from the philosophy that Dewey stands opposed to is that science both *draws on* and *refers back to* primary experience in order to obtain and verify the products/objects of its domain (ibid). Dewey presents his empirical method as a normative meta-philosophical position that should rely on primary experience in the same way that natural science does (ibid, p6). He makes clear that this empirical method both does allow us to properly understand the world, and that it is the *only* method by which we can come to understand the world (ibid, p2a). This latter point points to the fact that Dewey **endorses some form of metaphysics** in that he believes that we can truly come to know nature in the sense that scientific realists also advocate — science and empirical inquiry genuinely does tell us something about the world.

Dewey is also clear that what works in practice, and what we can verify and confirm through this primary experience, takes priority over what works in theory and in scientific theory. Theory is of course useful and essential, but ultimately 'ventures of this theoretical sort start from and terminate in directly experienced subject-matter' (ibid). This should apply not just to science, but also to all of us, to both 'the scientific man' (ibid) and the 'man in the street' (ibid). We can see this turn to practice also from his rejection, in his works on psychology, of the associationist psychologies that were popular at the time. The latter were based on Lockean/Humean accounts of psychology, which were very based in theory and introspection of the experiences that we have. Dewey opted instead for a psychology that was far more influenced by Biology and experiments (Hildebrand, 2018), and thus more based in practice. Dewey therefore fits more into the methodological empiricist framework in the **practice-led** characteristic. There is thus **no philosophy-first epistemology**, in accordance with ME. Regarding the practice-led characteristic, Dewey also endorses the pragmatic method that James sets out above.

Dewey criticises reductionist philosophies which compartmentalise phenomena and don't fully grasp the inter-connectedness of nature. This applies to the subject-object division that dominates Western philosophy, becoming especially prominent in Descartes. And thus to the idea of an epistemology that is focused around the individual subject and what that subject can and cannot know. Dewey, as were all the original pragmatists, is critical of this Cartesian notion of the self. Hildebrand writes:

'Dewey accepted from Hegel experience as manifested in particular social, historical, and cultural modes. Not only is the self constituted through experiential transactions with the community, but this recognition vitiates the Cartesian model of the simple, atomic self and methods based upon that presumption. Philosophy may start where we start, personally — with complex, symbolic, and cultural forms—and then articulate further emergences from them.'

(Hildebrand, 2018).

Dewey's empirical method seeks to dissolve this bifurcation and many others, whereas, he says, non-empirical methods that oppose his simply start with products such as the subject, sense-data, the mind, as if they were certain and primary when really this is not the case (Dewey, 1958, p9). For Dewey, and here we see his pragmatism, these sort of metaphysical issues can be boiled down to their empirical consequences in "primary" experience (ibid, p10). All of this shows that Dewey's empiricism is **not individualistic** in the traditional sense of EE.

Dewey also **rejects the centrality of sense perception** here, following James' model of radical empiricism. Dewey's conception of "experience" is far more all-encompassing than that of EE that we have seen so far, and allows as existing phenomena such as relations that cannot be directly perceived. These are, following James, nonetheless experienced. It's interesting to note also that Dewey's views on sensory perception reject it as being "given" in any sense in the way that others in the EE tradition use it.

2.3 The Vienna Circle, Logical positivism, and Logical Empiricism

Undoubtedly the greatest influence on empiricism in current philosophy of science, with Hume perhaps coming second, are the logical positivists/logical empiricists/the Vienna Circle. For reasons about to be made clear, these terms will be used interchangeably throughout this project. Although most people will now refer to the members of the Vienna Circle — Carnap, Frank, Hahn, Neurath, Schlick, etc — as the logical positivists, this name was coined later in 1931, and not by any of the main members but by Feigl and Bergmann, and arguably solidified by Ayer's *Language, Truth and Logic* in 1936 (Uebel, 2013). It was a term that was never really embraced by any members of the Vienna Circle. For instance, both Carnap and Neurath thought the association with positivism as potentially misleading, and would either refer to the Vienna Circle or logical empiricism when discussing their views (ibid). What's more is that from the 1930s the term logical positivism was used mainly only by those who were critical of the movement, and so consequently seemed to develop negative connotations around it. Further, there is no clear systematic usage between the differing terms of logical positivism versus Logical Empiricism (ibid, p85), although logical positivism is perhaps usually used synonymously with the philosophy of the earlier Vienna Circle and logical empiricism is far broader, and finds its origin in the Berlin school.

There is, to some degree, a modern understanding of the logical positivists that sees them as being crude empiricists, following directly and with only minor modification to the more radical EE of Hume, and then adopting Mach's phenomenalism. In addition, this modern understanding sees their project as very formalistic, as apolitical, and ahistorical. Styles and ideas of vital philosophers like Frank, Neurath, and Schlick are often missed out. This is due to many historical factors. One of which that most if not all historians agree on is the extreme popularity of Ayer's *Language, Truth, and Logic* in the English speaking world, which characterised the Logical Positivists in such a manner (Friedman, 1999)(Reisch, 2005). Reisch summarises this text as 'basically Carnapian philosophy of science viewed through the lens of Wittgensteinan ordinary language philosophy' (2005, p4). Ayer describes Logical Positivism's attitude towards philosophy as being the handmaiden of science, and to assist the progress of and perform clarificatory analysis on science. One illuminating passage from Ayer is:

'Thus the philosopher is not, qua philosopher, in a position to assess the value of any scientific theory; his function is simply to elucidate the theory by defining the symbols which appear in it'

(Ayer, 2014, p152).

Other factors leading to this more limited understanding of Logical Positivism include the characterisations given in Popper's *Logic of Scientific Discovery* (1934); criticisms from Quine in *Two Dogmas of Empiricism* (1951) and Kuhn in *The Structure of Scientific Revolutions* (1962); the characterisations of the movement made by Reichenbach (1936)(1938); and — Reisch claims — McCarthyist fearmongering in the U.S leading to the Unity of Science project (the area most connected to public facing and political elements) being dropped.

Unsurprisingly, the focus here will be on the logical positivist's empiricism.[12] Since this project is concerned with influences of current empiricism, and how current empiricism is thought of, both the true accounts of Logical Positivism and the cruder accounts that have taken hold of current philosophy will be explored. According to the latter view, the logical positivists are simply extending the tradition of the classical empiricism of Locke, Berkeley, Hume, and later on of Mach. There has been much good work in the last 40 or so years to try and reverse this view. To name just two important ways in which this has been done: there has been excellent scholarly work to show the logical positivist's neo-Kantian influences (e.g. (Coffa(1993) Friedman (1999)); and there has been similarly excellent scholarly work that highlights Neurath's contributions to the movement (which had typically been neglected in the orthodox picture) and overall demonstrate the socio-political dimension of the logical positivists (e.g. (Cartwright et al (1996), Reisch (2005), and especially the edited collection (Cat and Tuboly (2019))). The orthodox understanding of the logical positivist movement in current philosophy of science has understood them as paradigmatically belonging to EE through and through, following on from philosophers like Hume.

In the following part of this explication of logical positivism/logical empiricism, I will focus on Carnap, Hempel, Neurath, Reichenbach, and Schlick. I address each individually as opposed to characterizing the movement as a whole due to the fact that there are important and significant differences between each thinker's work. Most notably, whereas I categorize Carnap and Schlick as being largely in the EE tradition, I believe that Neurath more comfortably fits into the category of ME, although not as easily as someone like Bacon or Boyle.

Carnap

For Carnap's empiricism in the philosophy of science, I will be largely focusing on his *Logical Structure of the World* (henceforth *Aufbau*). This is the main text where he rationally reconstructs the empirical sciences. Although it was his earliest book (1928) in a long career of philosophy, Carnap writes in 1961 in the second preface to the *Aufbau* that he 'still agree[s] with the philosophical orientation which stands behind this book... especially for the

[12]For historical surveys that explore why it has come to be that Logical Positivism is seen as a movement that is ahistorical and apolitical, see Reisch (2005).

problems that are posed, and for the essential features of the method which was employed' (Carnap, 2003, pV). More importantly for this project, the key features that are characteristic of empiricism remain. The *Aufbau* also forms the beginning properly of the (in)famous protocol-sentence debate within logical positivism (Uebel, 2007, p25), which is an extremely useful insight into drawing out the empiricism of the various thinkers within the movement.

The *Aufbau* is an attempt to rationally reconstruct science and knowledge through a specific 'constructional system' (Carnap, 2003, p5) based on reducing sense experience to the phenomenally given. It is thus **theory-led** rather than practice-led, and this stays true for all of Carnap's career. Carnap, and the logical positivist movement more generally, is generally focused far more on theories within science as opposed to scientific experimentation. The **centrality of sense perception** is evident also from this — it is posited as the foundation from which his genealogy of concepts attempts to trace back to. The basic elements which form this foundation are "elementary experiences" (2003). Carnap later says that if re-writing, he would not use elementary experiences, but instead 'something similar to Mach's elements, e.g. concrete sense data' (ibid, pvii).

The project is also **individualistic**. The central idea behind it is to be able to develop this conceptual framework with the individual as central. Carnap considers two possible types of "basis" from which the system could be constructed — either physical or psychological — and opts for the latter. Within this latter category Carnap opts for the "autopsychological basis" (2003, p100), in which 'the available basic elements are restricted to those psychological objects which belong to only one subject' (ibid). This contrasts with a more general psychological basis, in which the basic elements of the system are 'the psychological objects of all psychological subjects' (ibid).[13] The most important reason for choosing the auto-psychological basis, as opposed to the general psychological or the physical, is that the constructional system being developed should incorporate a crucial epistemic component (ibid, p101). Epistemology runs central through this project; the *Aufbau* is presented straightaway as an attempt to construct 'an epistemic-logical system' (2003, p5) of objects or concepts.

Carnap's aim is also to entirely avoid metaphysics (thus satisfy the **no metaphysics** criterion of EE). His "construction theory" that is employed in the *Aufbau* is deliberately used so as to not require any metaphysics.

[13] Carnap is, though, clear that the world is 'identical for all observers' (2003, p7) and should be viewed as intersubjective and objective (ibid).

Metaphysics is 'beyond construction theory' (ibid, p15).[14] There are also other works by Carnap that make this fact more explicit. For example, his 1932 paper *The Elimination of Metaphysics Through Logical Analysis of Language* (republished in (1959)). In his 1928 *Pseudoproblems in Philosophy*, we see Carnap's version of the verification criterion — which the logical positivists are perhaps most famous for — which is intended to demarcate between metaphysics and non-metaphysics.[15]. The intention is to remove metaphysics from philosophical discourse. The essence of this is not really new. For instance, it can be found in Mach:

'[w]here neither confirmation nor refutation is possible, science is not concerned'

(Mach, 2013, p490)

Neurath

When orthodox expositions of logical positivism are given, it is Neurath's philosophy who is typically most neglected (Reisch, 2005)(Cartwright et al, 1996). Whereas Carnap fits more neatly into EE, Neurath fits less so. Neurath was incredibly focused on practice and scientific practice, and Cartwright — who I will subsequently characterise as the current archetypal figure in ME — frequently talks about Neurath's influence on her, and how her philosophy borrows from Neurath. In their jointly authored book on Neurath, Cartwright, Cat, Fleck and Uebel write:

'Neurath's philosophical positions are not derived by decisive arguments from first principles, a procedure which is altogether suspect from his own philosophical standpoint. Instead they are formed out of his immersion in day-to-day life and his attempts to understand and change the social structures around him. They ring of soundness and good sense, not of subtlety and depth'

(Cartwright et al, 1996, p4)

[14]Uebel (2007) also discusses this: 'On pain of the meaningless of all physical object discourse, it followed that all physical objects had to be reducible to phenomenal ones, for only if statements about physical objects could be reduced to statements about phenomenal objects could they be verified' (2007, p38).

[15]We also see a new criterion for eliminating metaphysics in Carnap's *Logical Syntax of Language* But nonetheless the intended outcome — the removal of metaphysics — is the same.

Thus here, I characterise Neurath as fitting more into ME.

Neurath's empiricism was firmly **practice-led**, and arose from properly engaging with the world around him. Pre-Vienna Circle days, he was involved in thoroughly empirical investigations into war-time economies; he was in charge of the economy in the short-lived Bavarian socialist state; he was heavily involved in various elements of "Red Vienna" — a political project that implemented many leftist ideas and programmes —, he ran museums in Vienna and helped to produce propaganda in the USSR. In his philosophy he also made choices based on what works in practice instead of what works in theory. For example, combining terms of both ordinary language and advanced scientific languages 'since in practice, the terms of both languages overlap' (Neurath, 1983c, p92).

Neurath was fiercely **against metaphysics**, though, in a way that fits him more into the category of EE. He advocated for a type of physicalism, which he believed steered him clear of metaphysics (e.g. (Neurath, 1959b)(Neurath, 1983b)). This physicalism simultaneously **rejected epistemology** (Cartwright et al, 1996, p6), or at least traditional forms of epistemology. He went so far as to believe that there should be no such thing/practice as philosophy, but only science. This has potential to mislead, though. He advocated certain types of philosophy, but only those that were continuous with science in some way. This type of philosophy, which he saw himself and others in the Vienna Circle as doing, he called 'anti-philosophy' (Neurath, 1983a, p48). Consequence of this disavowing epistemology, this fits more the characteristic of ME. Whilst ME simply focuses on not having a philosophy-first epistemology as central in one's philosophy, not having an epistemology at all certainly leans far more towards this than it does to focusing prominently on epistemology.

He also was not focused on the individual to the extent that the others in Circle were, and thus didn't try to reflect on scientific knowledge based on the personal experience of the individual in the way that Carnap and Schlick did. His 1932 (Neurath, 1959) contribution to the protocol-sentence debate argued against the individualist, autopsychological basis that Carnap presented in the *Aufbau* and *Unity of Science*. He argues that 'a defining condition of a sentence is that it be subject to verification, that is to say, that it may be discarded' (Neurath, 1959, p204); and that protocol sentences must be formulated so that anyone's protocol sentences can be verified by anyone: 'it makes no difference at all whether Kalon works with Kalon's or with Neurath's protocols, or whether Neurath occupies himself with Neurath's or with Kalon's protocols' (ibid, p207). He also writes that focusing too much on personal experience can 'decline into idealist metaphysics' (Neurath, 1983d, p101), and that to escape from this yet remain an empiricist he 'suggested avoiding the term "personal experience" and using the term "experience statement" instead.' (ibid, p101).

Nonetheless, Neurath did hold **sensory perception as central**, and saw it as the defining way in which one can confirm or reject scientific evidence. But we should be somewhat careful in asserting this. Neurath was a fallibilist and coherentist and thus did not think that there could ever be one central piece of evidence that was either (i) beyond reprisal or (ii) from which all of beliefs could be built from. Although protocol statements — the key pieces of evidence from which we justify theories in science — are confirmed via sense perception, '[t]here is no way to establish fully secured, neat protocol statements as starting points of the sciences' (Neurath, 1983c, p92). Contra Carnap, who believed that there are certain protocol statements that are not in need of verification, Neurath thinks that no statements are beyond this.

Schlick

Schlick certainly **held to some degree of scepticism** regarding the dismissal of metaphysics as he saw it in philosophy, but cannot be said to be a sceptic in the same vein as Hume or others earlier on in the EE tradition. Popkin (1992) points to Shlick's attempt to overcome this style of scepticism in Schlick's *General Theory of Knowledge* by asserting that the "unity of consciousness" is a brute fact that cannot be doubted (ibid, p256)(Shlick, 1974, p122).[16] Schlick also discusses this issue later, in 1932, where he argues against the global sceptic (Schlick, 1979b).

Schlick, along with Carnap and Neurath, placed **sensory perception as central**, therefore matching this characteristic of EE. The verificationism for which the logical positivists are well known is formulated in terms of being verified by sensory perception. Schlick writes that '[t]he act of verification... is always of the same kind: it is the occurrence of a particular state-of-affairs, *ascertained by observation and immediate experience*' (Schlick, 1979a, p157, my emphasis). These sort of references to sensory perception as key to verifying statements can be found all over his work; see for example (Schlick,

[16]See 'Now it appears that there actually is a fact on which we can rely here. It is more primitive than any doubt, more primitive than any thought. It lies at the base of all mental processes, it is directly given, it is a presupposition always fulfilled in consciousness. It is the plain, ordinary fact which we designate as the *unity of consciousness*' (Schlick, 1974, p122).

1979c)(Schlick, 1979b).

The verification principle is explicitly designed to distinguish metaphysics from non-metaphysics, of which Schlick (and all those in the Vienna Circle) attempted to eliminate from philosophy. There are many additional examples within Schlick's writings that one could give of him denouncing metaphysics in this sense.[17] Based on this we can therefore say that Schlick also adhered to the **no metaphysics** characteristic of EE. There are, though, some complexities to this assertion. First, in his pre Vienna Circle stage, Schlick adhered to a position of realism of some variety. Second, there were accusations against Schlick from Neurath of doing metaphysics with regards to Schlick's 1934 paper *The Foundations of Knowledge*. Neurath pointed to Schlick's correspondence theory of truth, and the way in which Schlick talks of 'absolute certainty' and 'agreement with reality', and in general the desire that Schlick places on statements corresponding to reality (Neurath, 1983d, p107-111).

Epistemology was at the heart of Schlick's empiricism, fitting suitably into EE. Although slightly before the formation of the Vienna Circle, his aforementioned monograph — General Theory of Knowledge — is, as it says, centred around providing a theory of knowledge. This interest in epistemology remains throughout his career. An article from 1932, based on a lecture he gave, based around the question "What can we know?" as a fundamental issue within philosophy.[18] In the same article, he also defines empiricism in the orthodox manner that I described in the first chapter: '[t]hose who do not believe that we can have any real knowledge that is derived from our reason but maintain that it must always rest on experience are called Empiricists' (Schlick, 1979b, p231). In Schlick's *The Foundations of Knowledge* epistemology agains takes centre-stage. The title of the paper points to this, but in addition (and as stated before) Schlick proposes a correspondence theory of truth. Schlick's empiricism is also **Individualistic**. The epistemological issues that he considers are from the perspective of an individual agent reflecting outwards into the world.

His empiricism and general attitudes towards philosophy have elements that are **theory-led** rather than practice-led. One example that draws this out is his "Turning Point in Philosophy" paper. Uebel (2007) writes that this 'must

[17]See, for example: 'If every view is to be labelled positivist, which denies the possibility of metaphysics, then nothing can be said against it as a mere definition, and in *this* sense I would have to declare myself a strict positivist' (Schlick, 1979c, p260).

[18]See (Schlick, 1979b, p225).

be read as his own variation on the collaborative manifesto' (2007, p77) that opposed the public manifesto of Vienna Circle — "Scientific World Conception: The Vienna Circle" — that Carnap, Hahn, and Neurath had written. Uebel says that Schlick was 'displeased with the socio-political overtones of the pamphlet' (ibid), conceiving of the movement instead as less concerned with practical matters such as these. Schlick's "Turning Point" sketches out how he sees the philosophy that will be the final stage of philosophy, and paints a picture that is generally less inclined to make philosophy the handmaiden of science than the manifesto. He sees philosophy as being concerned with meaning — both discovering and establishing meaning, not just with clarifying and doing work around the sciences (ibid, p78). Further, Schlick's concern overall is with scientific theory, and not scientific practice. Experiments and the logic of experiments are not discussed, nor is scientific practice generally discussed.

There are elements that don't seem theory-led, though. Schlick's stress and primary focus on the verificationist theory of meaning, for instance, — where for a statement to be meaningful and not nonsense it must be empirically verifiable — points to a focus on what works in practice as opposed to what works in theory. For example:

'By philosophical analysis we are unable to decide of anything whether it is real; we can only determine what it means to claim that it is real; and whether this is then the case or not can only be decided by the ordinary methods of daily life and science, namely, by experience'

(Schlick, 1979c, p263).

If Schlick is to be asserted as being "theory-led", then this differs from the theory-led empiricism that tended to dominate the Early Modern EE. Schlick was concerned with more practical applications than this.

Reichenbach

As Richardson and Uebel (2005) point out, despite the absence of philosophers of science who refer to themselves as logical positivists post 1970s or so, Reichenbach's work has never really gone out of fashion (Richardson and Uebel, 2007, p73). This may be due to Reichenbach's insistence that there was an important difference between logical empiricism — which he saw himself as — and logical positivism, which he saw as belonging to the Vienna Circle. Highly influential philosophers like Salmon (1979) certainly took Reichenbach's insistence to heart.[19] As previously mentioned, though, Uebel (2013) makes clear that there isn't really such a clear, sharp difference that we can draw.

Reichenbach, in *The Theory of Probability*, argued for a particular type of frequentist conception of probability which relied on idealizing the reference class to infinity (Reichenbach, 1949). This interpretation of probability was then applied to his epistemology later on in his life. Although it is here his epistemology that will predominantly be focused on, his frequentist conception of probability is also largely motivated by his empiricism.

Probability lies at the heart of his empiricism, going so far as to call his empiricism a 'probabilistic empiricism' (Reichenbach, 1961, pviii).[20] This probabilism included a type of fallibilism about knowledge which asserted that knowledge was only ever probable and could never be certain. He acknowledges that almost all empiricists have believed that knowledge is approximative, but asserts that the consequences of this have never been properly drawn out (Reichenbach, 1961, pvi). Reichenbach seems to have in mind empiricists of his immediate time, but this critique extends also to the EE and much of the ME that was seen in chapter one that it is typical of Early Modern times.

Reichenbach's probabilistic theory of meaning, which he sets out in the first chapter of *Experience and Prediction*, is foundational to understanding his empiricism. This is a specific version of the verifiability criterion that we see in the logical positivists above. He calls it the "probability theory of meaning" (1961, p54), and says that it consists of two principles:

First: 'a proposition has meaning if it is possible to determine a weigh, i.e. a degree of probability, for the proposition' (ibid)

Second: 'two sentences have the same meaning if they obtain the same weight, or degree of probability, by every possible observation' (ibid).

[19]For instance, Salmon opens his edited collection "Hans Reichenbach: Logical Empiricist" with the statement: 'Logical empiricism — not to be confused with logical positivism [...] — is a movement which has left an indelible mark on twentieth century philosophy' (Salmon, 1979, pix). And in Salmon (1999) he opens his paper on Hempel's philosophy of science by reiterating this distinction, arguing that Carnap's later philosophy, Hempel, and Reichenbach are amongst the most proponent philosophers of this school

[20]It is interesting to note that Patrick Suppes develops something he also calls 'probabilistic empiricism' (Suppes, 1980), but with no reference to Reichenbach.

Carnap, Neurath, and Schlick all take verification to be based around what we can directly observe with the senses. Reichenbach sees a problem with this inevitable anti-realism for the reason that it seems absurd that we cannot say that atoms, the interior of the sun, or electrons, only have meaning insofar as we can relate it to the human perceptual apparatus (1961, p46). The argument that he presents for his form of scientific realism rests on two concepts — "direct propositions" and "indirect propositions". The former are propositions of objects that we immediately observe, and the latter are propositions that we reach only through inference. Reichenbach's argument for scientific realism rests on trying to break down the distinction between the two styles of propositions. The argument is structured as follows:

P1) Even those facts which seem most simple and obvious to us actually rely on a variety of implicit or explicit inferences and thus are not absolutely verifiable in the sure manner that we typically treat them (ibid, p85 - 87). We can only really say that we know them with some probability.

P2) Thus, what we previously thought of as "direct propositions" are actually "indirect propositions" since they are never properly verified (p87).

P3) Since all propositions that stand to be verified are actually indirect propositions, the inferences that we make about unobservable entities are actually no different in kind to the inferences that we make in our daily lives about things that we can directly observe. (p90).

C: Therefore, we should not place any sort of significant epistemic difference between these types of inferences, and should accept unobservable entities that science discusses into our philosophy.

In (P1), and the pages that detail this premise, it may be tempting to assert that there exists some sort of epistemic scepticism in that Reichenbach entirely rejects the possibility of knowing anything with certainty. But this is better understood as a position of fallibilism as opposed to scepticism and thus as **not being a sceptic**. In that Reichenbach clearly wants to move away from attributing any special significance to phenomena which can be directly observed through sensory perception, we should see him here as matching the methodological empiricist characteristic of **de-centralising sensory perception** from the focus of their empiricism.

Whether or not Reichenbach's empiricism is **theory-led** or not is hard to say. The way that he arrives at some parts of his position is often through abstract philosophical contemplation, and to find the best theoretical solution to the problems that he sees with other empiricisms and to try and set up a suitable epistemology for science. But he will often refer to what scientists actually do in practice to motivate his philosophical decisions, or as the final say in judging a specific philosophical outcome.[21] Reichenbach is also sympathetic to the pragmatist movement, which was thoroughly practice-based. He writes that 'our conception may perhaps be taken as a further development of ideas which originated in pragmatism' (ibid, p69), but also believed that his empiricism could be seen as a 'further development of positivism' (ibid, p73), of which the latter was by and large more theory-led. With regards to the latter, though, Reichenbach makes it clear that the element that he draws from logical positivism is practical in essence:

'It seems to me that the psychological motives which led positivists to their theory of meaning are to be sought in the connection between meaning and action and that it was the postulate of utilizability which always stood behind the positivistic theory of meaning, as well as behind the pragmatic theory'.

(ibid, p73).

Reichenbach also takes the problem of induction seriously — which as has been already stated, is a thoroughly theoretically-driven problem — but then responds to it with a thoroughly pragmatic answer. Summarising, Reichenbach seems to lie somewhere in the middle of the two camps with respect to this point.

As has been seen, Reichenbach **endorses some sort of metaphysics** in that he epistemically commits to scientific realism, and thus to belief in unobservable entities and structures. He also commits to the reality of an external world. With respect to this characteristic he fits into ME.

Hempel

Hempel is surely one of the most influential figures in twentieth century philosophy of science. Nowhere, though, does he clearly and explicitly lay out any sort of empiricist position. It is typically claimed casually that Hempel is a logical empiricist — for instance, Fetzer (2017) and Salmon (1999) — and this is typically contrasted with logical positivism, by which they usually

[21]Here are some instances of this: (1) 'The choice, I think, cannot be difficult, as it has been already decided by the practice of science' (1961, p53). (2) '[t]he determination of this term is suggested by the method of verification used in the practice of science' (ibid, p47). (3) 'The justification of this expansion is given by the fact that this theory, and only this theory, corresponds to the practice of science' (ibid, p55).

mean the earlier work of Carnap and Schlick.

The focus of Hempel's work is in some ways orthogonal to much of the other work that has been highlighted by empiricists up until this point. Hempel's focus instead was largely on methodological issues in the philosophy of science. He wrote influentially on theory confirmation and induction, on the structure of theories, and most prominently on scientific explanation.[22,23]

The significance of the contribution that Hempel made to the field of scientific explanation is hard to overestimate. Of these contributions, perhaps most iconic his joint 1948 paper with Oppenheim — *Studies in the Logic of Explanation*. Salmon (1999) argues that this paper led to philosophers of science treating scientific explanation not only seriously but as a necessary topic to be engaged with, moving from a field that at the start of the 20th century was relegated to metaphysics.

But Hempel was nonetheless an empiricist. Empirical statements and a desire for them lie at the heart of all of his work in the philosophy of science. For instance, a necessary condition for the deductive-nomological model that him and Oppenheim propose is that an 'explanans must have empirical content' (1965e, p248); he stresses that the only sort of evidence that can confirm or disconfirm a theory is empirical (1965a, p22-24); and holds — with a lot of clarification and modification — to the general empiricist criteria of cognitive significance presented by earlier logical empiricists such as Carnap (Hempel, 1965c).

It is somewhat safe to say that Hempel **does not place sensory perception as central**. In his discussions of theory confirmation (1965a) that were mentioned in the previous paragraph, he restricts the conception of evidence that can confirm or disconfirm a theory to a specific type of empirical evidence that he says are akin in logical structure to the "protocol statements" that we saw of Carnap, Schlick and Neurath (1965a, p22-24). But, importantly, he stresses that it is far more liberal in various ways. Relevant to here is that they need not be restricted to sense-data, or even to what is observable via sensory perception. What is observable via scientific instruments is absolutely acceptable (ibid, p23), providing that certain specified techniques of observation have been agreed upon (ibid). In his discussions

[22]For all of these topics the best place to turn is his (1965), where essays on these topics are collected.

[23]It is from his work on theory confirmation, in particular (1965a) that the infamous "Ravens paradox" arises.

of the "empiricist criteria of cognitive significance", by which he again refers to the logical positivists criteria, he is in broad agreement but importantly phrases it with reference to "experiential evidence" (1965c, p101), as opposed to referring to sense-data, human experience, or any other term akin to the latter. And in his work on explanation, his necessary condition that requires empirical content is happy to classify whatever has been verified through experiment as constituting the empirical realm (1965e, p248).

Hempel can be said to be **theory-led** in a significant way, since all of his work focuses on scientific theories and not on scientific practice. We can turn to (i) his work on theory confirmation and (ii) his work on explanation. Regarding theory confirmation, the very topic of this is clearly theory-focused: it is the analysis of what confirms or disconfirms a scientific *theory*.

Hempel is **not a sceptic**, and thus does not satisfy this criterion of EE. He believes that genuine objective scientific knowledge does exist. In (Hempel, 1965b) he writes extensively about scientific knowledge with the obvious assumption that it exists. In addition, his work on explanation requires as a condition for the deductive-nomological model that the explanans, and indeed the entire explanation, be true (1965e, p248).

It is hard to say definitively whether or not Hempel **endorsed any metaphysics**. For instance: when discussing his deductive-nomological model of scientific explanation — despite the fact that this model gives laws a foundational role in explanation — he is relatively clear that his conception of a law is that of universal empirical regularities, and avoids concepts such as counter-factuals as being wrought with philosophical problems (Hempel, 1965f, p339). And his discussion of causality in explanation seems to display an anti-realist position towards causality as something above and beyond regularities (see Hempel, 1965f, p357-354). But he does seem more sympathetic to discussions of unobservable/theoretical entities than do earlier empiricists in the logical empiricist/logical positivist tradition, as was stated two paragraphs above; and he frequently talks throughout his work of truth, a concept that other empiricists in the EE tradition have not been willing to endorse.

His focus is certainly **not** on any **philosophy-first epistemology**, but nor is it — as per ME — really on **empirical investigation** into the world. It is in this sense that I have earlier stressed his tangential nature to other empiricists that we have seen in these two chapters.

Whilst broadly endorsing some sort of empiricist criteria of cognitive significance (1965c), he is firmly against a formulation of this on a subjective basis, and thus *cannot* be said to satisfy the **individualistic** characteristic of EE. He acknowledges explicitly (ibid, p103) the downfalls of formulating the criteria in such a way — namely that they cannot make statements about the distant past or distant future meaningful.

Conclusion

This chapter continued the history of empiricism that was begun in chapter 1. It began with various different 19th century empiricists and continued through to Reichenbach and Hempel. The aim of this chapter is to demonstrate the fact that there has existed an alternative version of empiricism to the empiricism that is typically used by current philosophers of science. Tracing the history of empiricism has done just this. The fact that those in the ME tradition in the period of history documented here are not typically seen as or listed as empiricists, despite holding to clearly empiricist doctrines, serves to illuminate this bias against empiricism that does not neatly fit into the orthodox narrative of empiricism, or does not correspond to EE.

Chapter 3

Empiricism in Current Philosophy of Science

Introduction

In the first chapter, the two different strands of empiricism, EE and ME, were explained, and the start of a history of these two positions was given. The second chapter continued this historical focus, and examined empiricism from the 19th century up until the mid 20th century, ending with Hempel. The first two chapters addressed the first aim of this thesis: the claim that there exists an alternative version of empiricism to what is typically associated as being empiricism within current philosophy of science, and that this alternative version (ME) has been heavily neglected in current philosophy of science. This chapter continues the theme of addressing this first aim, and concludes the first part of this thesis — what empiricism is, and what empiricism has been.

This chapter address current philosophy of science. By "current", I mean approximately from the 1970s onwards. I sketch out what EE and ME look like today by turning to the two most prominent advocates of each. This comes in the form of Bas van Fraassen's constructive empiricism coupled with his voluntarism (with respect to EE), and Nancy Cartwright's empiricism (with respect to ME). I then highlight the neglect of ME and exclusive focus on EE in current philosophy of science.

The structure of this chapter is as follows. First, in (3.1), I set out van

Fraassen's particular brand of empiricism — constructive empiricism — and his voluntarism. It is argued that although they are distinct, constructive empiricism relies foundationally on voluntarism for van Fraassen. It is thus more appropriate to analyse them as a whole entity as opposed to just constructive empiricism by itself. Then, this position is explicitly related to EE. In (3.2), Cartwright's empiricism is set out and it is stated how this fits into ME. (3.3) is concerned with the neglect of ME in current philosophy of science, and this is done in three ways. First, in (3.3.1) I examine van Fraassen's attempt to say what empiricism is, and what it could be, that occurs in his *The Empirical Stance*. Despite this being van Fraassen's attempt to explicate empiricism in the broadest sense of the term, I argue that it is not broad enough to sufficiently encompass ME. Given that van Fraassen is seen to be one of the, if not *the most*, iconic empiricists of current philosophy of science, this greatly contributes towards this neglect of ME. In section (3.3.2), I discus Clarke's (1999) analysis of Cartwright's empiricism. This is, to my knowledge, the only detailed engagement with Cartwright's empiricism, and Clarke only discusses Cartwright's empiricism through the lens of a very minimal concept of EE. Although he argues that Cartwright's empiricism does technically fit into this, I argue that this type of characterisation misses huge amounts of Cartwright's empiricism by not properly appreciating ME. The point is therefore to highlight the neglect of ME in understanding Cartwright's empiricism and to emphasise that fitting Cartwright into ME makes much more sense. Concluding (3.3), in section (3.3.3) a selection of quotations from philosophy of science over the last 40 years is presented that highlight the emphasis of EE and the neglect of ME in current philosophy of science.

I finish this chapter on (3.4) by reflecting on how and why this situation arose. That is to say, how and why it is that EE become synonymous with empiricism in philosophy of science. I argue that it is for four reasons. The first is down to the establishment of the orthodox narrative in the early 20th century; the second is the prominence of the logical positivists, who refer to themselves (perfectly acceptably) as empiricists; the third is the emergence of van Fraassen's particular brand of empiricism; and the fourth is a combination of the fact that what I am calling ME is relatively uncontroversial, and people believing empiricism to be a highly contested position.

3.1 Van Fraassen's Empiricism

It is important to distinguish between constructive empiricism, and empiricism more generally, as presented by van Fraassen. The former comes first, and is one way among many that one can formulate empiricism. His empiricism — which will be explored in the subsequent section in the discussion about the neglect of ME — is intended to be far broader and to encompass all forms of empiricism. Van Fraassen writes:

'There is within these constraints a good deal of leeway for different sorts of empiricist positions. For my part, specifically, I add a certain view of science, that the basic aim — equivalently, the base-line criterion of success — is empirical adequacy rather than overall truth, and that acceptance of a scientific theory has a pragmatic dimension (to guide action and research) but need involve no more belief than that the theory is empirically adequate'.

(Van Fraassen, 2008, p3).

It's also useful to say briefly here that voluntarism and constructive empiricism are two separate positions, but that constructive empiricism depends on voluntarism for its defence. Thus I claim that constructive empiricism, *as it currently exists*, is entirely dependent on voluntarism. Dicken (2010) argues for a version of constructive empiricism that does not use voluntarism as an epistemic foundation, but this does not pose a problem since my concern is with *van Fraassen's* constructive empiricism, and not constructive empiricism in a more abstract possibility. Voluntarism and how constructive empiricism depends on it will be discussed shortly.

3.1.1 Constructive Empiricism

Constructive empiricism consists of two complementary positions. The first concerns the aim of science, and the second concerns what it is for an individual to accept a theory in science. The first part will be referred to as the *"aims component"* of constructive empiricism, and the latter will be referred to as the *"acceptance component"* of constructive empiricism. Further, the acceptance component has two dimensions — an *epistemic* dimension and a *pragmatic* dimension. To accept a theory involves certain beliefs (epistemic) and certain attitudes and actions (pragmatic). Additionally, and somewhat contained in the pragmatic dimension of the acceptance component, constructive empiricism involves a *realist semantics* regarding the unobservable entities that science posits. It holds that these unobservable entities are not just referred to by scientists as "useful fictions" but are posited by scientists to be actual real entities. This can be called his "semantic realism", and it is here where constructive empiricism is most clearly differentiated from the instrumentalists and the logical positivists.

Van Fraassen's own concise definition is a good place to start to unpack all of this:

'Science aims to give us theories which are empirically adequate; and acceptance of a theory involves as belief only that it is empirically adequate'.

(1980, p12).

Missing from here is the pragmatic part of the acceptance component. This will be addressed shortly, but first the focus will be on the aims component of constructive empiricism. The claim that the *aim of science is empirical adequacy* has potential to mislead, so some clarifications are in order: the aim of science is *not* concerned with what the individual scientist believes the aim of science is, or with what the individual scientist motivations are for being a scientist; it is a statement about science as a collective enterprise. In addition, the claim is not intended to be a prescriptive claim about what the scientific enterprise *should* be, but instead a descriptive claim about what the scientific enterprise *is*. And finally, the aim of science (or for any discipline, for that matter) is identified with its main criterion of success. The following quotation demonstrates all three of these points:

'Some [scientists] do it, by their own testimony, in order to discover the plan of God's creation, and some to discover the true laws of nature; many more today do it to discover the structure of certain unobservable entities which they believe to exist. But the "it" that they do, I claim is work whose criterion of success in actual practice is empirical adequacy of the theories produced'.

(van Fraassen, 1994, p182).

Next is the acceptance component of constructive empiricism, with its epistemic and pragmatic dimensions. First, the epistemic. Acceptance of a theory 'involves belief only that it is empirically adequate' (1980, p12). This belief in empirical adequacy is both necessary and sufficient for accepting a theory as a constructive empiricist. But that is *not* to say that that's all that you're allowed to believe, if you are to be a constructive empiricist. One may certainly believe in other things in science, such as unobservable entities, and still call themselves a constructive empiricist; although this, van Fraassen would say, would be superfluous to what is required to the epistemic dimension of accepting a theory.[1] Qua scientist, you would believe only in the observable entities and suspend judgement on the unobservable, but qua individual you could believe anything you want (1989, p193-194). As an analogy regarding the superfluous nature of believing above and beyond what is required as necessary and sufficient, one can accept a scientific theory and believe an endless amount of other things that don't change one's philosophical stance on the matter — they could be a constructive empiricist and believe that they have free will, or that God exists, or that the way that they walk to work everyday is the quickest and most pleasant walk. All are compatible with but not really relevant to constructive empiricism, and this is the same (albeit much more extreme) with regards to believing in unobservable entities within science.[2]

Now, the pragmatic part. Van Fraassen writes:

'Acceptance of theories (whether full, tentative, to a degree, etc) is a phenomenon of scientific activity which clearly involves more than belief... if a scientist accepts a theory, he thereby involves himself in a certain sort of research programme... Thus acceptance involves not only belief but a certain commitment. [It] involves a commitment to confront any future phenomena by means of the conceptual resources of this theory. It determines the terms in which we shall seek explanations'.

(Van Fraassen, 1980, p12).

For the person who accepts the theory, to be a constructive empiricist they must (at least) believe that the theory is empirically adequate, and they will act *as if* the theory that they accepted is true in certain respects.[3]

[1]Van Fraassen makes this point in (1994), where he says that both gnostics and agnostics can be constructive empiricists, although the gnostic about unobservable entities would perhaps be missing the spirit of constructive empiricism as a position, and in (1989) on several occasions

[2]See: 'it is not part of constructive empiricism to say that the adoption of such additional beliefs is irrational — just that it is more than what is involved in scientific theory acceptance' (van Fraasen, 1994, p182). For more on this see also Rosen's (1994) paper that discusses this in depth, and van Fraassen's (1994) response, where much of this is explicitly clarified.

[3]Rosen (1994) gives an excellent account of theory acceptance in constructive empiri-

For the person who has accepted quantum field theory and the standard model of particle physics, they will offer explanations of how particles, atoms and molecules stay together (as opposed to breaking apart) through this framework, and answer that it is a combination of the strong nuclear force and electromagnetic force, but they need only *believe*, minimally, that the theory is empirically adequate. They are able to, as stated above, believe more (i.e. believe in these unobservable entities) and still be constructive empiricists, but this would be surplus to requirements and miss the spirit of what it is to be a constructive empiricist.

In *Laws and Symmetry*, van Fraassen presents a version of theory acceptance intended to apply specifically to irreducibly probabilistic theories.[4] This begins with modifying "belief" into a more subtle 'gradated opinion, modelled as personal probability' (1989, p194). It involves accepting the objective probabilities that any theory in science gives us about the theory as a whole, but in terms of how we hold our own personal probability, we need only hold this subjective probability for what the theory says about the observable. This personal probability is formulated via Miller's principle, which states that our personal probability should reflect the physical probability that the theory gives us (van Fraassen, 1989, p82). Formally:

$$P(A|ch(A) = x) = x$$

Where "ch" means 'chance'. In addition to being not required to hold gradated opinion about the unobservable entities/structures of the theory — i.e., accepting it without believing it — Fraassen also treats the notion of physical probability, or objective chance, with the same regard of accepting it for the purposes of holding to his own subjective notion of probability, but does not actually require belief in it.

cism, and Dicken (2010) gives an excellent account of the distinction between "acceptance" and "belief".

[4]He also discusses theory acceptance of irreducibly probabilistic theories in (1980), writing that: 'a statistical theory is empirically adequate if it has at least one model such that the difference between predicted and actual frequencies in the observable phenomena is not a statistically significant difference' (p196). But, as stated in (2007, p338), believes that this is an inadeqaute account.

3.1.2 Voluntarism

A first clarificatory point is that what is being discussed here is *not* voluntarism in the sense that one can choose at will what to believe, or believe whatever one wants in the sense of being able to simply will ones self to do so. Rather, it is a voluntarist theory of rationality, of sorts, that forms the foundation of his constructive empiricism.

Voluntarism takes its roots in probabilistic epistemologies, and van Fraassen cites Pascal as a chief influence.[5] This means that beliefs and sets of beliefs should not be evaluated in terms of a distinction of "true" or "false", or any such similar binary options, but should be assigned degrees of belief from the agent about statements. For example: the statement "it will rain tomorrow" is best evaluated not by any definitive agreement or disagreement but with a certain degree of belief assigned to it. This is not unique to voluntarism — for instance, Bayesian epistemologies also take this as a central tenet.[6]

Van Fraassen directly contrasts his voluntarist epistemology with both Bayesian epistemologies (1989) and more "traditional" epistemologies (1989)(2000), where traditional epistemologies are characterized by van Fraassen as including rules of induction and inference to the best explanation (IBE) as central imperatives by which one should adhere to in order to be rational (1989, p151). Addressing Bayesian epistemologies first: voluntarist and Bayesian epistemologies actually have much in common.[7] The essential difference between Bayesian conceptions of rationality and voluntarist conceptions of rationality is that the Bayesians have a slightly stricter criterion of what makes one rational; for the Bayesian, to not logically update your sets of beliefs via Bayes' theorem is to render one irrational. This is not required for the voluntarist. One can refuse to update their beliefs in light of new evidence and still be considered rational. Van Fraassen expands on this by giving us four claims that the sceptic would make, all of which is from (1989, p178):

(1) There can be no independent justification to believe what tradition and ourselves of yesterday tell us.

(2) It is irrational to maintain unjustified opinion.

[5] (1989, p152-153)

[6] Voluntarism actually has much in common with Bayesianism, and van Fraassen spends time on direct criticisms of Bayesianism (1989) to contrast it with his position.

[7] See Psillos (2007) for an excellent comparison

(3) There is no independent justification for any ampliative extrapolation of the evidence plus previous opinion to the future.

(4) It is irrational to do (3) without justification.

The Bayesians would, according to van Fraassen, agree with all *except for* (2). The voluntarist would, instead, agree with (1) and (3) but disagree with (2) and (4).

Moving on to criticisms of rules based around general ampliative inferences and specifically IBE: van Fraassen denies that we must adhere to ampliative forms of inference (including especially IBE), and that one is free to reject any and/or all ampliative inferences that they see fit and still be rational (providing that they adhere to a set of minimal criterion that are presented below). To be clear: van Fraassen is *not* claiming that to adhere to ampliative inferences or the IBE via some sort of rule is irrational. Rather, he is saying that if one does not follow these rules, or even abstains altogether from them, doing so does not necessarily render one irrational.

Now for the positive extrapolation of voluntarism. It is hopefully seen from the previous discussion that voluntarism is a highly permissory theory of rationality that allows for an individual to hold any beliefs, and for those beliefs to hang together in any such manner so long as a few criteria are met. The beliefs (held together) must be (a) probabilistically coherent, (b) not logically contradictory, and (c) avoid self-sabotage. Note that for all of these, they must apply to beliefs held not just synchronically but diachronically also. It is hopefully clear what (b) means, but perhaps not so for (a) or (c). Beliefs being probabilistically coherent entails that one not subject oneself to a dutch-book style scenario regarding one's beliefs where, in betting terms, one is guaranteed to lose money.[8]

Regarding (c), van Fraassen explains that an act be assessed in two ways. Evaluating it prior to the act, we assess how 'reasonable' (1989, p157) it is, and post-act we can assess to what extent it was 'vindicated' (ibid). The "no self-sabotage" principle is the criterion that 'you should not sabotage your possibilities of vindication beforehand' (ibid). When it comes to belief in certain future events, you should not act in such a way as to stop the chances of these events occurring. See the following elaboration from van Fraassen:

[8]For a far more detailed and technical elaboration of probabilistic coherence, see (van Fraassen, 1989, p318-348).

'If your aim, in giving commands to Peter, was that he should give a present to Paul, then you are vindicated if he does. Suppose you give him the two commands, to give Paul a horse and to give Paul nothing. Then you have given commands which cannot be jointly satisfied — so the vindication will necessarily something to be desired. Similarly, if your aim, in making factual descriptive statements, is to give true information, then you are vindicated if your statements turn out to be true. Should you make several mutually incompatible statements, they *cannot* be jointly true, so your vindication will necessarily be less than total'.

(van Fraassen, 1989, p157).

3.1.3 Constructive Empiricism's Dependency on Voluntarism

Constructive empiricism *as it currently exists*, is entirely dependent on voluntarism for its sustainability. Whilst it's potentially possible to formulate constructive empiricism without voluntarism — Dicken (2010) argues for a version of constructive empiricism that hinges on a different epistemic foundation to voluntarism — this is emphatically *not* van Fraassen's actual formulation of things and so does not pose a problem here. The focus here is on the actual, not the possible; it is on how van Fraassen conceives of constructive empiricism. The establishing of this claim is essential to my classification of van Fraassen's empiricism as falling into EE as opposed to ME, and also in a claim in the subsequent chapter that there are internal tensions with van Fraassen's brand of empiricism.

It can be shown that constructive empiricism is dependent on voluntarism by examining both the "big picture", macroscopic level, and the "small picture", microscopic level. Regarding the big picture, van Fraassen sees constructive empiricism only as a viable alternative to scientific realism, and at no point does he claim that one should not believe scientific realism, or that it is irrational to commit to scientific realism/believe in unobservable entities. *The Scientific Image* is best read as an attempt to show why one is not rationally compelled to believe in scientific realism, through a series of arguments both showing why one need not believe in it, and showing positions that one can believe in their place (namely, that of constructive empiricism) that are just as viable. The opening of it stresses this: '[t]he aim of this book is to develop a constructive alternative to scientific realism' (1980, pvii). This overall strategy makes far more sense if we assume that it is a voluntarist foundation that is being used.

Next, van Fraassen is and has been open to the charge of being selectively sceptical about what he argues in favour of believing or not believing, with respect to his constructive empiricism. This is made sense of if we understand that from the beginning, van Fraassen has developed constructive empiricism with this voluntarist foundation in mind. For instance: the drawing of the observable-unobservable line around the level of sensory perception and the insistence that accepting a theory only requires one to believe in its empirical adequacy. Both seem arbitrarily placed in their scepticism — the former with respect to the level of sensory perception and the latter that one need only believe in the part of scientific theories that one can observe with the human eye. Hacking (1985) criticises this selective scepticism on the basis that there are no good grounds to consider unobservable phenomena as less reliable than observable phenomena, since we obtain data around unobservable phenomena through several different sources which we combine together as evidence. Churchland (1985) says that observable claims in scientific theories should also be a target of our scepticism due to these being similarly banished from our ontologies. Both come to opposite conclusions — the former to be less sceptical, the latter to be more sceptical — but the point of both is that van Fraassen is being somewhat arbitrary in what he advocates belief for. But this makes complete sense in light of his voluntarism; van Fraassen doesn't really need to justify his beliefs in such a strong way, since his voluntarist epistemology is so weak in what counts as rational. He is perfectly justified, within this framework, to believe in both drawing the observable-unobservable line at this level, and in asserting that when one accepts a scientific theory one need only believe in its empirical adequacy.

There are various more localized instances — the "small picture" — that can be pointed to that make this foundational role of voluntarism more explicit. These specific points within constructive empiricism either (a) make sense only in light of, or (b) make *more* sense in light of, having a voluntarist foundation. Here are some of them:

(i) When discussing the observable/unobservable division, there is no concrete justification that this is *the* correct way to view the situation. He writes that it is not 'rationally compelling' (1980, p18) on either side of the debate, and concludes the defence of his position as boiling down to the fact that 'it is, on the face of it, not irrational to commit oneself only to a search for theories that are empirically adequate' (ibid, p19).

(ii) His arguments for constructive empiricism do not rest on the most iconic arguments for anti-realism — namely, the pessimistic meta-induction or the argument from underdetermination:

'I am quite proud never to have relied on the so-called Pessimistic Induction either, any more than on this Argument from Underdetermination — though the former has also at times, quite wrong, been associated with *The Scientific Image. Neither would be at all in harmony with the views I went on later to defend in epistemology, but whose beginnings are, as Van Dyck documents, traceable from The Scientific Image onwards.*'

(Van Fraassen, 2007, p347)

The point here is that van Fraassen doesn't use these arguments because he doesn't *need* a justification for the positions he holds in constructive empiricism in light of his voluntarism. Further, in the usual way of understanding both the argument from underdetermination and pessimisic meta-induction, both arrive at a normative conclusion of some sort — that we ought not to believe in/posit as true unobservable entities. Van Fraassen holds no such normative position, consequence of his voluntarism.

(iii) The very fact that he doesn't want to commit to a normative position on whether or not one should or should not believe in unobservable entities is a reflection of his voluntarism. Recall that constructive empiricism is not a position on what one *should* believe with respect to unobservable entities, but just a position that says you do not *have* to believe in the unobservable entities.

(iv) As pointed out by Van Dyck (2007), van Fraassen explicitly denies in (1980) that there are epistemic rules which can force belief/disbelief on us:

'A complete epistemology must carefully investigate the conditions of rationality for acceptance of conclusions that go beyond one's evidence. What it cannot provide, I think... is rationally compelling forces upon these epistemic decisions'

(van Fraassen, 1980, p72-73).

3.1.4 Situating Constructive Empiricism into the Tradition of Epistemic Empiricism

It has been shown that constructive empiricism, as formulated by van Fraassen, has a voluntarist foundation. It will now be shown how constructive empiricism, with this voluntarist foundation, fits into the characteristics that were given of EE earlier.

(1) Individualistic: Experience is considered from the perspective of the individual human observer.

We can see this by turning to how he conceives of theory acceptance in science, and frames his voluntarism. As was seen above, van Fraassen discusses theory acceptance from an epistemic and pragmatic perspective. Both are focused around the *individual*, and not around the scientific community. What is required from one when one accepts a theory is framed solely around the individual, and involves both beliefs (the minimal belief that a theory is empirically adequate (van Fraassen, 1980, p12)), and a practical commitment to confront phenomena within the framework of this accepted theory (ibid). This by itself is enough to place van Fraassen firmly as displaying this characteristic, but his voluntarism adds a further dimension here. As has been seen above, the voluntarist epistemology is displayed entirely around what is rational *for the individual*. Group beliefs are not taken into account in the analysis at all.

(2) Centrality of Sense Perception: Experience and observing is construed in terms of sensory perception.

Again, theory acceptance within constructive empiricism is concerned with the individual human and argues that they must minimally commit only to what is observable for the human with the naked eye, and believe only that the theory is empirically adequate. Much of the controversy around van Fraassen's constructive empiricism involves exactly that he places the focus of the observable around sensory perception, and gives epistemic privilege to it.

(3) Epistemic Scepticism: EE is heavily influenced by scepticism of various epistemic kinds.

This is perhaps most controversial. Van Fraassen would deny being a sceptic (1989, p176-182; 2000). This seems at least defensible, given that constructive empiricism is motivated by his voluntarist epistemology: the minimalist position of constructive empiricism is not, at least according to van Fraassen, motivated negatively by a desire for more certain belief, but positively via voluntarism. But constructive empiricism can be said to be *in the spirit of* scepticism. The "spirit of scepticism" that I mean here can be summed up well by what Westphal (2021) calls van Fraassen's 'sceptical tropes' (2021, p8). These are, amongst others, (1) his selection of empirical adequacy over theoretical truth, (2) his appeal to a Darwinian, practical account of the success of scientific theories (van Fraassen, 1980, p40); (3) his appeal to and reliance of pragmatism; (4) his concession to subjectivism in (van Fraassen, 2008); his "phenemenology of science" (2008, p83). These are attitudes or positions that he shares with the sceptical tradition.

(4) Theory-Led: EE is based far more theoretically than practically (what works in theory over what works in practice), and in philosophy of science based far more on theories in science than on scientific practice.

The central focus of constructive empiricism isn't about going into the world and investigating, but is about both theory acceptance and trying to understand what we can know through theories. In *Quantum Mechanics: An Empiricist View*, van Fraassen investigates what the world would look like if quantum mechanics were true. This is done through focus on theory and formalism, not on practice.

(5) No Metaphysics: EE does not involve unobservable structure or entities, where unobservable is demarcated with reference to the human observer.

Van Fraassen is perhaps more subtle here than other empiricists more generally, and would not make any sort of normative claims about whether we can or cannot have knowledge of unobservable structure or entities. But constructive empiricism need not believe beyond the observable realm, and the spirit of constructive empiricism certainly matches this. If constructive empiricism, via *The Scientific Image*, matches the spirit of this point, then in *Laws and Symmetry* it is made mor explicitly. Although not directly a book arguing for constructive empiricism, it makes a positive argument against positing laws of nature and against belief in natural necessities. As will be seen in (3.3.1), in *The Empirical Stance* van Fraassen also insists that empiricism, more broadly construed, can be characterised as 'a recurrent rebellion against metaphysics' (van Fraassen, 2002, p36). This is meant to be a characterisation of *all* empiricist positions — but clearly, in virtue of constructive empiricism being a special type of empiricism, this anti-metaphysical characterisation also applies to constructive empiricism.

3.2 Cartwright's Empiricism

3.2.1 Scientific Empiricism

Cartwright calls her empiricism *Scientific Empiricism*(Bristol Centre for Science and Philosophy, 2021b). And true to the name, it is concerned with methodology and not with regular epistemological issues. Whereas van Fraassen's constructive empiricism is set out in a systematic way over the course of *The Scientific Image*, with various bits qualified over the years, this is not the case for Cartwright. Cartwright's empiricism is spelled out over the course of her philosophical career and most concretely in (ibid). The following is thus an attempt to construct her empiricism in a systematic way that has not been explicated by her in print.

There are three main components and then several further characterisations and qualifying remarks that can be made:

(1) An Axiology: Science should teach us the empirical facts (ibid, 1.39). This is axiological in the same sense as constructive empiricism — for van Fraassen the aim of science is empirical adequacy; for Cartwright the aim is to give us the empirical facts. There are two salient differences that should be recognised. (i) Although empirical adequacy and empirical facts are seemingly similar, for van Fraassen empirical adequacy concerns the observable-for-humans. For Cartwright, empirical facts are not dependent on the human at all and include all objects in space and time that are exhibited by taggable features, relations and kinds. (ii) Cartwright's claim is a prescriptive claim about what science *ought* to do (ibid, 1.11), whereas van Fraassen's claim is intended as a descriptive claim about what science *actually does*.

We can see Cartwright's insistence on empirical facts as reminiscent of first Bacon's and then Boyle's insistence on natural histories as providing the foundational framework for knowledge. For them, the natural histories are a compilation of empirical facts about a certain phenomena. We can also see a remarkable similarity here between Cartwright's conception of empirical facts and Boyle's notion of "matters of fact" (see 1.4.1).

(2) A Methodology: If we want to know what science tells us about the world, we should turn to the empirical facts themselves — we should turn to empirical investigation of the world. We should *not* go in with a priori assumptions and impose these onto our investigation e.g. that the world is unified, that universal determinism exists, that the world will be a certain way, etc. This methodology is fairly pervasive in her various projects, but is made more explicit in *The Dappled World*:

'[G]uarantee nothing *a priori*, and gather our beliefs about laws, if we must have them at all, from the appearance of things.'

(Cartwright, 1999, p12).

There are many instances where this is reflected, but two can be turned to for now that highlight this well. The first is her anti-realism about fundamental laws because they do *not* give us empirical facts about the world (Cartwright, 1983, p54-74), and her realism about phenomenological laws because they *do* give us empirical facts about the world (ibid, p100-128).

The second is a central thesis of *The Dappled world* — that is her rejection of the claim that quantum physics has come to replace classical physics. Classical physics still works and still gives us empirical facts. And it does this better than does quantum physics does at certain scales. Therefore it cannot be said to be replaced by quantum mechanics in any real sense when it comes to our treatment of various scales. See, for example:

'My studies of the most successful applications of quantum theory teach me that quantum physics works in only very specific kinds of situations that fit the very restricted set of models it can provide; and it has never performed at all well where classical physics works best.'

(Cartwright, 1999, p2).

(3) A Principle of Conservativism Regarding Ampliative Inferences. We have to use ampliative inferences in science.[9] But when doing so, we shouldn't make big leaps — when we're doing them, take "baby-steps" and don't speculate (Bristol Centre for Science and Philosophy, 2.25). In *The Dappled World* Cartwright argues against extrapolating beyond what is shown to be true in highly specialised contexts — for example, in experiments — onto the world more generally. Early on she gives an example of the standard model of particle physics. If it makes solid empirical predictions which

[9]'One cannot do positive science without the use of induction' (Cartwright, 1999, p24)

accord well with the theory, without modifying various parameters, then all is fine with allowing an inductive inference that this provides good evidence that the standard model is true *in situations that are relevantly similar to the experiments that have been made.* It's not fine, according to Cartwright, to extrapolate beyond this and say that it would work for situations in which we haven't yet tested it, or that the results can be applied in situations that differ in important respects to the conditions of the LHC:

'We have virtually no inductive reason for counting these laws as true of fundamental particles outside the laboratory setting — if they exist there at all.'

(Cartwright, 1999, p34)

This can also be seen in *How the Laws of Physics Lie* (HTLPL) on the topic of explanation from laws. Laws *do* explain in special circumstances, but science usually generalises this explanation beyond being specifically an explanation for the circumstances in which the explanation is applicable. We get an explanation from an ideal circumstance, and we keep that explanation for cases even where the ceteris paribus law does not apply to:

'The pattern of explanation derived from the ideal situation is employed even where the conditions are less than ideal; and we assume that we can understand what happens in nearly isotropic media by rehearsing how light rays behave in pure isotropic cases... [this] is an assumption, and an assumption which... goes well beyond our knowledge of the facts of nature'

(Cartwright, 1983, p48).

We can also see this principle echoed in *Nature, the Artful Modeller*:

'I urge firm empiricism: Short steps from what we see to what we claim there is, not high flights of fancy or great leaps of faith.'

(Cartwright, 2019, p39).

Remark 1: ***There is a focus on practice rather than theory, and on what works in practice rather than what works in theory.***

The focus that Cartwright places on scientific practice rather than theory reflects not just her empiricism, but her wider philosophy of science and also how she conceives of the more important elements of science more generally. In this practice-led focus, she follows in the tradition that has been referred to as the "Stanford School" (Cartwright, 1999, pix), which contains philosophers such as Patrick Suppes, Hacking, Dupre, Galison, and Morrison. Whilst there is certainly a general turn in current philosophy of science towards practice, including that of van Fraassen's more recent work, this focus on practice in Cartwright's work is at the heart of everything, and really drives her empiricism.

Cartwright holds that (a) the correct place to turn in order to understand the world is scientific practice, not scientific theories; and (b) that we should be concerned with what works in practice, not with what works in theory: 'I am an empiricist. I know no guide to principle except successful practice' (Cartwright, 1999, p2). Both (a) and (b) fall naturally out of the **axiology** and **methodology**: *if* science should be about empirical facts, and *if* we should turn to empirical facts to find out what science tells us about the world, then it logically follows that both (a) we should turn to scientific practice rather than theories to learn this, and that (b) we should find out what works in practice rather than what works in theory. Clearly what works in theory is not in the domain of empirical facts if it does not work in practice; and equally clearly what works according to scientific theory is not in the domain of empirical facts unless it has been shown to be so by scientific practice.

Remark 2: ***No priority given to the human observer, or to sense-data.***

This can be clearly understood when we realise that both the **axiology** and **methodology** outlined above understand empirical facts *not* as observer-dependent but as taggable things that exist. There is nothing in Cartwright's empiricism that requires priority given to the human observation; in fact in various places she explicitly argues against it. For example:

'Many of the things that are realities for physics are not things to be seen. They are non-visual features — the spin of an electron, the stress between the gas surface, the rigidity of the rod. Observation — seeing with the naked eye — is not the test of experience here'.

(Cartwright, 1983, p7).

And:

[The concern] 'is not to ground science in pure observation or in direct experience, but rather to ensure that claims to scientific knowledge are judged against the natural phenomena themselves. Questions about nature must be settled by nature — not by faith, nor metaphysics, nor mathematics, and not by convention or convience either'.

(Cartwright, 2000, p47)

Remark 3: ***A focus on singular facts over general facts. We derive general claims from singular claims, and not vice-versa.***

This makes complete sense when we consider Cartwright's axiology as outlined above. Science should tell us about empirical facts, which are singular; and if we want to know about generalisations in the world then we should generalise from the singular empirical facts.

This attitude underpins all of Cartwright's philosophy, but two explicit examples can be pointed to: The first is in HTLPL, where she argues in the second chapter *against* the covering-law model and instead argues that we move from particular facts and generalize these into ceteris paribus laws.[10] The second, and more explicit, is in NCATM, where she argues against Hume's method of moving from general claims about causality to singular claims about causality, arguing instead that the correct way to do this is the opposite (Cartwright, 1989, p91-140) — to move from singular claims about causality to the more general.

3.2.2 Situating Cartwright's Empiricism into the Methodological Tradition

(1): Empirical Investigation. Focus is still on experience but on using experience to investigate the world. Advocates an empirical/empiricist method whereby empirical investigation is the best way to proceed.

This is seen above via principle 2: her methodology. Cartwright explicitly advocates turning to the empirical facts themselves to know about the world, rather than making any other assumptions.

(2): De-centralisation of sensory perception. There's a general shift away from a focus on sensory perception, and so consequently a lack of focus

[10]'in fact, most of the high-level claims in science are *ceteris paribus* generalizations' (Cartwright, 1983, p44).

or dismissal of the significance of the "observable-for-humans". The focus is instead on what we can measure or on what we can learn through experiment.

With Cartwright, the shift is from naked-eye observation to "empirical facts". This is seen explicitly in "remark 2" above.

(3): Mild scepticism. There exists some sort of scepticism, but typically far less radical than that of the epistemic version. It tends to be local and not global, and not so severe as to reject the possibility of any metaphysics.

Cartwright is evidently not a global sceptic in the sense of many in the EE tradition. She frequently talks about being able to access the world and makes claims about scientific knowledge with no questioning of whether this scientific knowledge is possible. There is certainly a mild scepticism, though; this can be seen clearly in her aforementioned principle of conservativism regarding ampliative inferences.

(4): Practice-led. Focused far more on scientific practice rather than scientific theory, and on what works in practice rather than what works in theory.

This is covered identically in "remark 1" above; there can be no doubt that Cartwright's empiricism matches this characteristic.

(5): Willingness to endorse some metaphysics. The kind of metaphysics that is compatible with this is something like a purely naturalistic metaphysics that is justified by directly appealing to scientific experiments or practice.

Causality and capacities play a large role in Cartwright's philosophy. In HTLPL, Cartwright argues against inference to the best explanation and in favour of inference to the best cause, and also argues that causal laws are indispensable to physics.[11] Her (1989) is a book centrally about capacities and how we need capacities to be able to make sense of science, and provides extensive discussions of this.

The way that Cartwright conceives of experiments also relates to this. In Cartwright's views, when we perform experiments we are learning about the nature of the phenomena that we are experimenting on; we are learning about the capacities of it. This relates closely to predecessors in the methodological tradition, namely Bacon and Boyle and their heavily favouring *luciferous*

[11]See chapter four for the former, and chapter one for the latter.

experiments over *fructiferous* experiments. The former are types of experiments that allow us to discover real causes and axioms, whereas the latter simply yield practical outcomes (Anstey, 2014, p112).

(6): Shift away from philosophy-first epistemology.*Philosophy-first* epistemology does not play a central-role; any epistemology done is justified through this empirical method.

Cartwright does not engage with any sort of philosophy-first epistemology, or epistemology at all for that matter. Cartwright is focused instead on the world itself (Cartwright, 1999). In this sense, Cartwright follows in the footsteps of Neurath (see 2.3).

3.3 The Neglect of Methodological Empiricism

Thus far, I have given extensive coverage of ME in order to demonstrate both its existence and historical significance. Next, I show it's neglect in current philosophy of science. In what follows, a selection of evidence is laid out that shows that philosophers in current philosophy of science typically treat empiricism as synonymous with EE, or at the very least formulate empiricism so that ME is not compatible with it.

3.3.1 Van Fraassen's Account of What Empiricism Is

First, I examine van Fraassen's account of what empiricism is, and what it could be. The point of this in the current context is to show that the way he characterises empiricism is not broad enough to allow for ME, and thus to show that this is contributing towards a neglect of this version. The fact that van Fraassen is the most influential empiricist in current philosophy of science and fails to account for anything resembling ME concretely demonstrates ME's neglect.

Van Fraassen's presentation of his empiricism, distinguished from his constructive empiricism as highlighted above, can be found in various articles — (1992)(1994)(1995a) — in segments, and in its most fully developed form in his *The Empirical Stance* (2002). Van Fraassen opposes his empiricism against "transcendental" empiricism (see (1994)(1995a)(2002)) and any sort of empiricism that is foundationalist. Van Fraassen has one main argument for this that is comprised of two parts; the argument is presented in greater depth in (1994) and (1995a), and more concisely in (2002). The argument is essentially this: empiricism cannot have a main tenet or dogma, on pains of contradiction. The *first* part of this argument is that empiricism holds strongly to scientific modes of inquiry and rationality and a key part of this includes being able to disagree about various principles. But any central dogma/tenet/principle in empiricism wouldn't be open to dispute and would therefore contradict this attitude that is at the heart of empiricism.[12]

The *second* part of this argument varies slightly depending on where the claim is made, but the key part of it remains the same. Van Fraassen begins by taking a typical, albeit naive, formulation of empiricism: *Experience is our one and only source of information*. The only way that one can confirm this, acting as an empiricist, is surely to assume this to be an *a posteriori* statement, because an empiricist could not have an *a priori* principle at the heart of their philosophy, given their rejection of any *a priori* claims to non-logical, non-tautologous truths. In (1994) his argument is that if this statement is *a posteriori*, then it must be open to scientific investigation. But there's no way in which this statement could be both prepared in such a way as to be suitable for scientific investigation and also play the role of empiricist dogma.[13]

Instead, van Fraassen proposes that empiricism is instead a *stance* that holds no core tenets, axioms or dogmas; rather, to be an empiricist is to hold certain attitudes and ideals that are chiefly (i) respect for scientific modes of inquiry, (ii) rejection of metaphysical inquiry, and (iii) seeing as virtue the idea of rationality that doesn't bar disagreement (which is derivative of (i) in the sense that van Fraassen views this as being a key part of scientific inquiry).[14] Both (i) and (ii) are crucial for the subsequent parts of this essay. Expanding on (i), van Fraassen does not intend for us, as empiricists, to rely on the *findings* of science, since they will very likely will change, but rather to the way in which scientists go about their findings. He writes:

'Empiricism may also be approached through reflection on its positive attitude toward science. But this admiring attitude is not directed so much to the content of science as to their forms and practices of inquiry. Science is a

[12]See (2002, p38-46)(1994, p310-314).
[13]See (1994, p314-317)
[14]See (2002, p61-63).

paradigm of rational inquiry. To take it as such is precisely to take up one of the most central attitudes in the empiricist stance. But one may take it so while showing little deference to the content of any science per se... How do we live in a world in which, to the best of our knowledge and belief, all our best most fundamental scientific theories are false? We live in it by the light of science as practice, as search, as rational form of inquiry par excellence'.

(Van Fraassen, 2002, p63).

Van Fraassen also, qua empiricist, rejects the demand for explanation as holding highest rank amongst philosophical virtues. He does so in (1980) by rejecting the claim that a good explanation should be any indicator of truth (p,23-34); and this claim forms a fairly foundational element in his (1989) where his rejection of the laws of nature is motivated largely by this:

'When a philosophy – as many do – raises explanation to pre-eminence among the virtues, the good pursued in science and all natural inquiry, he or she really owes us an account of why this should be so. What is this pearl of great price, and why is it so worth having? What makes laws so well suited to secure us this good, when laws give us satisfying explanations, in what does this warm feeling of satisfaction cost?'

(Van Fraassen, 1989, p31)

In some respects, it seems like van Fraassen's characterisation here is broad enough to allow for both traditions of empiricism, EE and ME. He explicitly says that empiricists should be focused on scientific inquiry and not the products of science, and leaves the stance seemingly purposively general to allow for wider scope. But there are three important indicators that his account of empiricism is not broad enough to encompass the ME version.

The first two points can be seen with the following quotation: 'empiricist philosophers have always concentrated on epistemology, the study of knowledge, belief and opinion, *with a distinct tendency to advocate the importance of opinion*' (Van Fraassen, 2002, pxviii, my emphasis). This reflects the trait in EE of epistemic scepticism — a scepticism towards knowledge as certain, with the emphasis instead being on beliefs and opinions. This is certainly the case for EE, but not so for ME. Francis Bacon's, Boyle's, and Whewell's empiricism revolves around at least some parts of knowledge as certain, and Cartwright talks frequently about our ability to have genuine knowledge of the world itself. Second, for van Fraassen to place the stress on epistemology in this quotation seems clearly indicative of this EE tradition, rather than ME. Whilst ME certainly can incorporate and focus on epistemology, and indeed many in the tradition do, it typically takes second-focus next to methodological inquiries.

Third and finally, van Fraassen also argues that empiricism involves a "strong anti-metaphysical" trend (ibid, p31). This is, again, certainly true of EE, but certainly not so for ME. As has been seen in the project so far, a central component of ME is that it allows and emphasises metaphysics of a naturalistic variety — a metaphysics that is directly based on and drawn directly from the results of empirical science.

3.3.2 Clarke on Cartwright

Relatively little has been written on Cartwright's empiricism. An exception is Clarke's "Empiricism, Capacities, and Experiments" (1999). Clarke's understanding of Cartwright's empiricism, though, understands empiricism only in terms of EE, and attempts to place Cartwright's empiricism inside this. This is thus indicative that current philosophers of science simply conflate empiricism to EE. I discuss this in order to further highlight the inability of curent philosophy of science to discuss ME, or any style of empiricism outside of this EE trend.

Clarke first says several things that I am in complete agreement with: that Cartwright's empiricism is 'a long way from Humean empiricists' (Clarke, 1999, p363); that 'Cartwright... is best understood as an empiricist who is offering a rival version of empiricism to standard Humean formulations' (ibid, p364); and 'the empiricist tradition has always been broader than the narrow neo-Humeanism which it is often thought of as being synonymous with' (ibid, p370-371). Whilst it looks like Clarke is close to recognising the non-homogeneity of EE, his proposed alternative is to broaden empiricism only enough to sufficiently step out of the Humean version, but not outside of the EE version. He uses Carruthers' understanding of empiricism, which is a textbook characterisation of EE:

'[T]he empiricist tradition is to be understood as being contrasted with rationalistic philosophies, and there are two aspects of rationalist thought which empiricists have traditionally wanted to oppose. First, empiricists have wanted to oppose all claims that there may be such a thing as innate knowledge. Second, empiricists have wanted to oppose any claim to the effect that there can be substantive knowledge of the world which is a priori'.

(ibid, p371).

Clarke focuses on the latter — empiricism as wanting to oppose any claims to substantive a priori knowledge of the world. First, Clarke argues that this claim is equivalent to saying 'that we should be as open-minded as possible about what the world is like. If we are open-minded about the possible contents of the world then, by default, we will base our beliefs about the way the world is, as much as possible on empirical evidence' (ibid). This is false, though — the two statements are not equivalent. The former, orthodox understanding of EE is a definitive statement that there can be *no such thing* as substantive a priori knowledge of the world; Clarke's reformulation is that we should base our beliefs about the way the world is on empirical evidence *where possible*. The logical differences are that the former is a universal statement whilst the latter is evidently not. The semantic differences are that one (orthodox formulation of EE) is about a wholesale rejection of a certain type of knowledge whilst the other (Clarke's re-formulation) is about holding beliefs that correspond to empirical evidence where possible.

Neither Carruther's orthodox formulation nor Clarke's apparently equivalent formulation are an accurate characterisation of Cartwright's empiricist project. Starting with the orthodox EE formulation first: Cartwright is not concerned with epistemic claims about whether or not we can have knowledge of the world which is a priori. She would certainly want to assert that if we want to know what science is telling us about the world, we should turn to the empirical facts alone and *not* make a priori assumptions — but this is very different to this characterisation of empiricism that is assigned here. Cartwright's claim that seems similar is different in kind in that hers is entirely methodological whilst this understanding is epistemic.

Taking Clarke's re-formulation, the fit is still not there. The focus is still on epistemology, and on how our beliefs should be justified and/or warranted. For the same reasons as in the previous paragraph, Cartwright is not concerned primarily about beliefs or about knowledge, and her empiricism is certainly not focused epistemically in this manner about where one should base their beliefs (Bristol Centre for Science and Philosophy, 2021b)(Cartwright, personal correspondence). Cartwright is concerned prescriptively with what science and scientists should be doing, methodologically, when they do science; and with what science should be about. She is simply not concerned with epistemology — with what the individual or the collective should or are justified in believing with respect to science. She is even not concerned with what the scientists themselves should be, or are justified in, believing, asserting that 'scientists can believe what they wish' (Bristol Centre for Science and Philosophy, 2021b, 2.45) — providing that their *actions* adhere to the aforementioned principle of conservativism regarding ampliative inferences in science. *What one believes does not matter for Cartwright; it is their practical actions that matter.* This is the crux of the problem with both formulations that are mentioned here.

We can see this confusion arising due to a failure to take into account that there are other alternatives to this EE version that dominates our understanding. Clarke clearly acknowledges that Cartwright's empiricism does not fit into an orthodox empiricism that is typically aligned with Hume, but fails to conceptually move outside of EE and recognise that an alternative form of empiricism exists — namely, ME — that Cartwright's empiricism exists within. Clarke is not alone on this though. Clarke and other philosophers of science seem to be working within a milieu that simply doesn't recognise any empiricism existing outside of EE.

3.3.3 Selected Quotations from Current Philosophers of Science

Here, quotations and pieces of evidence are listed that support my claim that current philosophy of science, and thus current philosophers of science, largely fail to recognise an alternative version of empiricism to EE, and view empiricism as comprising solely of EE. Much of the evidence displayed in this sub-section is based around philosophers using a contrast between realism and empiricism, thus implying that the two are at odds, and that empiricism is an inherently anti-realist tradition. But the anti-realist version of empiricism is, as has been shown, to be confined to the EE tradition. ME is typically realist in character, and should certainly not be globally characterised as being anti-realist. If anti-realism is talked about as being a vital characteristic of empiricism, then it is EE that is being discussed and not ME. The following displays selected quotations in current philosophy of science from important philosophers, displaying that they see empiricism as being exclusively EE, and do not take ME into consideration.

First, and unsurprisingly, is van Fraassen. In the opening page of *The Scientific Image*, he directly contrasts empiricism with realism:

'The opposition between empiricism and realism is old'

(1980, p1).

Further, in the rest of the introduction he constantly reinforces this division between realism and empiricism without any reference to any other sort of empiricism that may exist. The iconic collection of essays that came out a few years later in direct response to constructive empiricism in *The Scientific Image* was titled: "Images of Science: Essays on Realism and Empiricism", with there evidently being stated that there is a contrast between the two. This edition features essays which, unsurpisingly, contain more of this dichotomy, thus restricting empiricism to EE. For instance, Hooker (1985) writes: 'Empiricists... hold that the depths [beyond perception] are ghostly, having no independent rationale for their veneration beyond the surface through which they indirectly appear' (Hooker, 1985, p153). Giere writes: 'I would not enter this battle if I did not feel strongly that realism is right and empiricism wrong' (Giere, 1985, p75).

In the Routledge Companion to Philosophy of Science (2008), Psillos and Curd write:

'The renaissance of scientific realism in the 1960s resulted in an epistemic optimism with regards to science's claim to truth, though new forms of empiricism emerged in the 1980s'

(Psillos and Curd, 2008, pxxv).

Again, it is here implicitly suggested that empiricism rejects claims to truth that science makes. In this same edition, Sober — in the entry in this encyclopedia on "empiricism" — writes of the empiricism that is central to philosophy of science being an empiricism that contrasts with scientific realism (Sober, 2008, p129). He goes on to characterise this empiricism as having a 'preoccupation with sense experience [that] takes the form of a thesis about the role of observation in science' (ibid).

Psillos, in a chapter directly addressing Cartwright's empiricism, writes:

'Where many philosophers have thought that these two positions [empiricism and realism] are incompatible (or at any rate, very strange bedfellows) [...] Cartwright tries to make a case [for the combination of the two]'.

(Psillos, 2008, p167).

Two passages from Massimi can be pointed to:

‘Observable phenomena have become the hallmark of empiricism and the threshold beyond which the elusive realm of unobservable entities — dear to scientific realists — begins’

(Massimi, 2007, p238)

‘Current philosophy of science has been characterised by a lively and ongoing debate between two positions: realism and empiricism’.

(Massimi, 2010, p153).

Bueno’s entry in the Routledge Handbook of Scientific Realism (2018) focuses exclusively on EE, with no mention of characteristics that appear in the ME tradition but not in the EE tradition, nor reference to iconic thinkers in ME. Bird, in his forthcoming monograph, launches a fierce attack on empiricism, claiming that we cannot have an empiricist account of science, yet focuses exclusively on EE. He characterises a vital component to empiricism as being observation through sense perception:

‘A moderate but nonetheless significant strand of empiricism remains as a standard component of many philosophers of science: observation is the foundation of scientific knowledge — evidence is observational in nature — and observation is primarily a matter of sense perception’.

(Bird, forthcoming, p4).

Dicken, albeit focusing the book on constructive empiricism, will refer to empiricism more broadly as being anti-realist and contrasted with scientific realism (e.g. Dicken (2010, p13). Hans Radder (2021) argues that scientific practice requires causality, but that empiricism cannot allow for this; this fact, he says, damns empiricism. Clearly here the concept of empiricism that he is working with is that of EE — he defines empiricism from the get-go as holding a basic assumption that ‘sensory experience constitutes the foundation of all knowledge and that belief in the reality of unoservable entities, properties, events or processes cannot be epistemologically justified’ (Radder, 2021, p598).

And finally for now, Teller writes:

‘van Fraassen, in keeping with the spirit of empiricism, is skeptical about abstracta and about modalities as facts about the world’

(2001, p131)

There are many more examples that one can find in the current philosophy of science literature to illustrate this point, but I believe that these will suffice.

3.4 Why Has This Happened?

Up until now I have simply highlighted the fact that ME has been neglected, and that EE has emerged as the dominant version of empiricism that philosophers of science by and large tend to treat as synonymous with empiricism. But no explanation has been offerred as to why this is the case. This brief section offers a partial explanation for the dominance of EE and the relative neglect of ME.

First, a more specific characterisation of just what current philosophers of science tend to see empiricism as, specifically. I stand by the view that they characterise empiricism as something at least similar to EE, if they are pushed. But as can be seen from the previous section, three points in particular stand out — (i) a position of anti-realism (ii), a rejection of metaphysics, and (iii) a drawing of the observable-unobservable line around the level of sensory perception. This provides important insight into three partial explanations.

(1) As discussed in (1.1), the establishment of the orthodox narrative of empiricism presents a very crude version of EE. As stated, this came to be the consensus of what empiricism is somewhere between 1895-1915 (Vanzo, 2016) and this is the narrative that remains as canon, at least in pedagogical terms. Most people are aware that treating empiricism as limited to Locke, Berkeley, and Hume is overly simplistic and idealized, but they think that the essence is correct and thus that despite being more complicated than the story makes out, it is basically correct. I agree that it is basically correct, *but only about EE.*

(2) The sheer dominance of the philosophy of the Vienna Circle, who always referred to themselves as empiricists. It has become a sociological fact that the philosophy of the Vienna Circle has been interpreted them in popular philosophical culture as being phenomenalistic empiricists. This was discussed in (2.3). As already stated, despite the crude portrayal of their

philosophy that has come to dominate, they nonetheless — with the exception of Neurath — remain in this EE tradition. In this period of philosophy of science — from around the 1920s-1950s — there was no-one as prominent as these figures who called themselves empiricists. This creates a sense of synonymity of empiricism with these figures who label themselves as empiricists. In addition to this, the orthodox narrative of empiricism in point (1) is firmly established by the 1915. Taken together, it makes sense that people neglect other forms of empiricism that may exist.

There were many objections to the logical positivists/logical empiricists. But a significant school that emerged in direct reaction to these thinkers were scientific realists. This movement rejected (i) (a position of anti-realism), (ii) (a rejection of metaphysics) and (iii) (a drawing of the observable-unobservable line around the level of sensory perception) in their advocation of scientific realism. This movement emerges in the 1960s with iconic papers from Maxwell (1962), Sellars (1962) and a book from Smart (1963), and arguably gains traction with philosophers such as Boyd (1973)(1980), Hooker (1976), Putnam (1975), Smart (1968). Interestingly, empiricism is not treated in this way — i.e. by characterising it specifically as EE or with emphasis on (i), (ii), or (iii) — in Smart (1963), Boyd (1973), Maxwell (1962), Sellars (1962), Putnam (1975). But it *is* treated in this manner by Hooker (1976), who treats empiricism in much the same way that current accounts of empiricism treat it. It is referred being an 'alternative to Realism' (ibid, p410), and makes various other statements about empiricism that treat it as being characterised exclusively as EE. Given this fact, it seems that (2) alone cannot be a sufficient explanation, since pre 1980s it was not the norm to characterise empiricism in such a manner. But (2) nonetheless seems to set the stage for this conflation of empiricism to EE.

It should be importantly stressed that Reichenbach also rejects points (i), (ii), and (iii), yet is still considered as an empiricist and one that fits into this tradition. I do not have a good account of why this is, apart from to treat his case as an anamaly.

(3) The emergence of van Fraassen's particular brand of empiricism, which is by far the most influential empiricist position in current philosophy of science. Van Fraassen's brand of constructive empiricism coupled with voluntarism is — as has been seen in this chapter — importantly different to the empiricism of the logical positivists/logical empiricists. And is certainly far more sophisticated. But the consensus that EE is synonymous with empiricism is really made concrete here, since a far more sophisticated position

of empiricism is presented that *still nonetheless holds to the central tenets of EE*. Especially to (i) a position of anti-realism (ii), a rejection of metaphysics, and (iii) a drawing of the observable-unobservable line around the level of sensory perception. Thus, the only alternative to the dominant empiricism of the logical positivists/logical empiricists nonetheless retains these features, and cements into philosophy of science this phenomenon of viewing empiricism only as EE.

(4) What I class as ME is now relatively uncontroversial as a position within philosophy of science, and so philosophers simply don't associate it with or see it as empiricism. Empiricism has been ingrained into the status quo of philosophy of science to be controversial — partially as a combination of points (1), (2), and (3). When a position is seen as relatively uncontroversial, which upon reflection can plausibly be said to be empiricist in some important way, the connection is not made since this would conflict with the controversial nature that has been tied to empiricism.

Conclusion

This chapter is the third chapter in the more descriptive part of the project: what empiricism is, and has been. This chapter had two aims, both of which correspond to the first aim of this overall thesis. The first of these is to present the current most prominent manifestations of each version of empiricism — EE and ME. This took the form of the constructive empiricism coupled with voluntarism of van Fraassen, and the empiricism of Cartwright. The second aim has been to highlight the neglect of ME within current philosophy of science. The first aim was achieved by explicating both van Fraassen's and Cartwright relevant work on the topic, and making clear exactly how they both fit into these respective versions. The second aim was achieved by pointing to reflective instances within current philosophy of science that are symptomatic of this neglect, and provide clear instances of what I mean when I say that ME has been neglected. This came in the form of (i) van Fraassen's account of empiricism more broadly, which is supposed to account for all empiricism but fails to account for ME; (ii) Clarke's analysis of Cartwright's empiricism, which analyses it in terms of EE and fails to recognise that there is any alternative to EE, and thus gives an insufficient analysis of Cartwright's empiricism; (iii) selected quotations from prominent philosophers of science where they equivocate empiricism as a whole with EE, thus neglecting ME from their considerations.

This chapter also concludes the first part of the thesis. It is consequently worth taking stock of what has been established so far. This part has aimed to firmly establish that there exists — and has always existed — an alternative form of empiricism to EE. This is a form of empiricism that is not sufficiently captured by EE. And to show that this form of empiricism, ME, has been heavily neglected by current philosophy of science. It is worth stressing here that the figures that exist within the tradition of ME are not minor figures who can be brushed away as insignificant. This is true both throughout the history and now. Whilst the historical issue is of high importance here, this is not something that is confined to history: this is an empiricism that exists now and is embraced by one of the most prominent figures in current philosophy of science, Nancy Cartwright. From a purely descriptive point of view, this is of great significance.

The subsequent part of this project argues that embracing some form of ME is crucial for any empiricism that wishes to exist within current science — that is to say, for any empiricism that can exist as a suitable philosophy of science within current science. It is therefore not just of significance from the perspective of wanting to better understand philosophy of science; empiricism must embrace ME as a far better suited type of empiricism than EE.

Part II

What Empiricism Should Be

Chapter 4

Normative Criteria for an Empiricism in Philosophy of Science

Introduction

As has been seen in part I, those who developed ME — with the exception of the pragmatists — were heavily concerned with the methodology of science. Bacon developed a new method for achieving knowledge of the world, which we now see as an ancestor of the scientific method. Boyle, Hooke, and others from this time within the Royal Society take on this approach. In the 19th century we see Whewell, Herschel and Jones declare themselves Baconians and take on the project in a form more relevant to their times. Thomson has clear views on how science should be performed, and draws from Herschel. Cartwright does the same, proposing explicitly a normative methodology for science. Part I explored the neglect of these positions in current philosophy of science, and briefly explored why this has been the case.

Part II considers what results from taking ME seriously in application to current science. It thus explores the great potential of ME as a position within philosophy of science, whilst also arguing that formulating empiricism this way is how empiricism *should* be formulated. There is thus both an exploratory and a normative dimension to this chapter.

This chapter presents four normative criteria for a viable empiricist position that are derived from the characteristics of ME. Unsurprisingly, given that Cartwright fits very neatly into ME, there is much overlap with her empiricism. There are, however, novelties that arise from taking ME seriously in this way, which Cartwright either doesn't consider or considers but doesn't develop. These are explored subsequently.

The first criterion is that it must allow for the collective nature of science. It is argued here that epistemic individualism is not a suitable position for achieving this task. I first spell out what I take epistemic individualism to be by appealing to Antony's (1995a) characterisation of this, and then systematically argue against it.

The second is that it should be able to embrace the shift that has occurred in philosophy of science towards an increase focus on scientific practice as opposed to predominantly holding a focus towards theories. I very briefly detail the historical transition from theory-focused to a more practice-focused shift in philosophy of science, and then outline the reasons for this and why this is a welcome move and should be part of any empiricism suitable for current science. I then take into account the fact that van Fraassen, in his later work, has focused more on scientific practice, but argue that there is a tension between what his views are on scientific practice that he sets out via his conception of what empiricism is, in the broadest possible sense of the term, and his voluntarism.

The third is that empiricism should not give epistemic privilege to what is observable by the naked eye, but should instead epistemically privilege the class of "measurables". I briefly explore traditional motivations for understanding the observable in terms of naked sensory perception for philosophers, but show that van Fraassen is largely not motivated by this. I then, by appealing to Musgrave's (1985) and Ladyman's (2000) criticism of van Fraassen, show that the only internally coherent way to draw the observable/unobservable line is very different to EE. For this distinction to remain coherent it must either, as Ladyman (2000) argues regarding constructive empiricism, incorporate modality, or the class of what is taken to be "observable" must be limited to only what has been *actually* observed. The latter is clearly not suitable for science, and so whilst internally coherent must be disregarded. I also take into account practical considerations regarding epistemically privileging this class of observables, and present arguments and case-studies presented by Hacking (1985), Shapere(1982), Evans and Thébault (2020), and Massimi (2007). A natural solution to this problem is to extend the line of what is observable to that which can be "observed" through scientific instruments. This does not go far enough, I argue, and instead propose that it is the class of phenomena that is measurable that should be epistemically privileged. I advocate that the phenomena that are measured in the first part of a measurement process should gain greatest epistemic privilege, and explain for the reasons for this in the section.

The fourth is that it should incorporate a realist view of causality. I argue that in virtue of the above criterion that we should welcome the shift in philosophy of science to a more practice-based approach, we should favour such approaches. In actual scientific practice, and *not* according to what theoretically is the case, we do have and operate with causality in science. Causality is vital to a working, practical science. I draw heavily on the works of Hacking (1983) and Radder (2021). I also rely upon the third criterion here — that of the epistemic privileging of measurement — and argue that measurement cannot properly be made sense of without a realist notion of causality.

4.1 Characteristics of Methodological Empiricism

Recall briefly the characteristics of ME that were given in (1.1.2):

(1): Empirical Investigation. The focus is still on experience but on using experience to investigate the world. An empirical/empiricist method is the best way to proceed.

(2): De-centralisation of sensory perception. There's a general shift away from a focus on sensory perception, and so consequently a lack of focus or dismissal of the significance of the "observable-for-humans". The focus is instead far broader.

(3): Mild scepticism. There is some sort of scepticism, but typically far less radical than that of the epistemic version. It tends to be local and not global, and not so severe as to reject the possibility of any metaphysics or notion of knowledge.

(4): Practice-led. The focus is far more on scientific practice rather than scientific theory, and on what works in practice rather than what works in theory.

(5): Willingness to endorse some metaphysics. The kind of metaphysics that empiricists of ME endorse is something like a purely naturalistic metaphysics that is justified by directly appealing to scientific experiments or practice.

(6): Shift away from philosophy-first epistemology.*Philosophy-first* epistemology does not play a central-role; any epistemology done is justified through the empirical method.

Note that the two versions — ME and EE — are not opposed in all respects. ME primarily focuses on using an empirical method to learn about the world (Bacon, Boyle, Hooke, Whewell, Herschel) or to normatively claim that this empirical method should be used for science (Cartwright); EE primarily focuses on providing an empiricist epistemology.

ME can clearly incorporate epistemological elements. Although Cartwright's focus is not on epistemology at all, Francis Bacon was concerned with implementing a method that would secure us knowledge. Boyle and Hooke talk about knowledge, and take a somewhat sceptical attitude towards the certainty of knowledge. Whewell and Herschel develop their own epistemologies that fit into this ME framework. And we can imagine other forms of ME where epistemology plays a role. The important point that was emphasised in characterising ME is that the epistemology is not a *philosophy-first* epistemology. This takes us up to the point where the first criterion can be formulated.

The purpose of this chapter is to derive, from the characteristics of ME, an empiricism that is best suited to current science. These come in the form of four normative criteria than an empiricism should meet. This chapter explores the potential of ME as a philosophical position appropriate to current science, and argues also that this is the best way to formulate empiricism. These characteristics of ME have persisted since the advent of science, and have been held by various individuals and communities who have made great scientific and philosophical breakthroughs. Science has advanced a great deal since then, though, and becoming almost unimaginably more complex than when Bacon was writing. Nonetheless, I think it is relatively fair to say that some form of the criteria that I will present would be broadly agreed with if presented to any of those in the ME tradition, with the possible exception of the pragmatists, who were concerned less with scientific methodology and more with metaphilosophical methodology.

4.2 Collectivism, not Individualism

The first criterion is this:

(1) Any empiricist position in current philosophy of science must not rely on epistemic individualism, but on a collective epistemology of some sort.

We can take this from several of the characteristics of ME: (α) **empirical investigation**, (β) **de-centralising of sensory perception**, (δ) **practice-led**, and (γ) a **shift away from philosophy-first epistemology**. In order: (α) successful empirical investigation into the world that hopes to maximise success requires a collective effort, not an individual one. Anything we learn from this is not dependent only on the actions or knowledge of an individual, but of a collective. A collective epistemic framework is thus required to be presupposed here. (β) when those in the ME tradition do not want to place emphasis on sensory perception of an individual and move away from the importance of this, an important factor in this is qua individual. The sensory perception of an individual is open to a great number of errors; the sensory perception of a collective, less so; the results of an experiment that has been performed collectively and repeatedly, even less so. Whilst not of direct relevance, there is absolutely an indirectly important relation that can be drawn from this characteristic. (δ) When we examine scientific practice, and what happens in practice in the world, people learn collectively and not individually. (γ) A shift away from a philosophy-first epistemology, in the context of an empiricism applied directly to science, requires adapting an epistemic framework from the practice of science, and not imposing an epistemology prescriptively onto science. It is descriptive of science rather than prescriptive onto science. When we derive this epistemic framework from science, then we get epistemic collectivism, not individualism.

Science is undeniably a collective enterprise where both the activity of science and the products of science are performed by and result from, respectively, not the individual scientist but the scientific community.[1] Despite historical narratives of science often focusing around individuals, even the figures that are touted as being *the* great scientists who made breakthrough discoveries seemingly by themselves — e.g. Galileo, Newton, Curie, Einstein, Darwin, amongst others —, could not have done so without large amounts of

[1] I take this distinction between the processes and products of science from van Fraassen (2002, p155

help from both their predecessors and contemporaries.[2] And as well as scientific discovery, the community and collective element applies also to every element of science. Confirmation of scientific discoveries, theory confirmation more broadly, performing experiments, taking measurements, publishing papers (to name just a few) are actions that are performed by communities and not individuals; individuals cannot do science in isolation. This goes back to the beginning of scientific experiment as we think of it now: Schapin and Schaffer (1985) document how a crucial component of establishing scientific "matters of fact" in the mid 17th century included allowing others in the scientific community to witness the results of the experiment, and convincing others that they were indeed factual.

The main argument of this subsection is that any form of empiricism that is to be considered suitable for science cannot rely on epistemic individualism, and thus a collective notion is instead needed. To make this case, the field of social epistemology is turned to and relied on. Social epistemology takes the epistemic focus away from the individual and onto the community. It also takes into account the social environment, which is typically idealized away in more traditional accounts of epistemology.

To argue that epistemic individualism isn't suitable for science, it must first be made clear just what epistemic individualism is. This comprises the first part of this section, and I do this by using Antony's (1995a) summary. Antony presents three characteristics that define this position. The first two of these, I will argue, can be dismissed relatively straightforwardly as unsuitable for any empiricism in the context of science and thus little time is spent on them. The third characteristic is more complex than the other two; consequently, more time is spent arguing against it.

The third characteristic in question is "methodological individualism", which involves the view that the individual is the "primary epistemic subject". I explore what it means to be a primary epistemic subject; to do so I briefly introduce a few ways that beliefs can be considered (genealogically, methodologically, ontologically), and claim that whether or not the primary epistemic subject is individual or collective depends on this category being

[2] The focus on individuals in history, and of history being the "history of great men", is reflective of the "Great Man Theory" of history which emerges in the 19th century largely through Thomas Carlyle. This way of doing history has become less popular since, but nonetheless seems to stick in popular imagination. The focus on individuals more broadly is perhaps a reflection of the Modern age that we live in, which is ideologically characterised by liberalism, which focuses around individuals and not collectively.

able to present convincing cases for the majority of these ways of considering beliefs. They are then addressed in turn — first, ontologically, then genealogically, and then methodologically — and a case is made, based on these, for the requirement of the collective as the primary epistemic subject, not the individual, at least insofar as science goes.

4.2.1 What is Epistemic Individualism?

To define epistemic individualism we can turn to Antony (1995a). She lists three main theses that "traditional", namely individualistic, epistemologies propose:

(1) The interchangeability thesis: 'Individual knowers are epistemically interchangeable... [T]here is no epistemically relevant variation among individual knowers' (p63).

(2) The self-sustainability thesis: 'Human beings do not require interaction with other human beings in order to acquire knowledge' (ibid).

(3) Methodological individualism: 'The individual is the primary epistemic subject' (ibid).

Whilst more defensible as an in-principle claim in general epistemology, the self-sustainability thesis (2) in modern science is not attainable, either in principle or in practice. This has been the case for the last few hundred years of science at the very minimum. There is so much background knowledge that a scientist of any discipline must come to know, which comes from other humans; and a would-be-scientist cannot hope to become a scientist without extensive interaction with the scientific community.

(1) — the interchangeability thesis — is often criticised by feminist epistemologists for the fact that this has typically been presumed by male philosophers in positions of power and privilege, and thus does not accurately reflect all knowers but only them. Antony (1995b) argues that this is problematic for a feminist project, because it presupposes that there exist proper differences between, for example, men and women, rich and poor, which seems to be the very claim that it foundationally frames itself against. I do not intend to make any comments on such general statements as these, but only concern myself with science. In science, the interchangeability thesis is unobtainable. Many scientists have different mindsets and ways of thinking that cannot

be presumed to be captured from the standpoint of an individual epistemic agent trying to capture a universal scientific epistemology. Kuhn (1977) discusses something akin to this with his notions of the convergent mindset and the divergent mindset, whereby the convergent mindset is essential in periods of "normal" science, and the divergent mindset is essential in periods of revolution.

(3) is more complicated, and cannot be dismissed as straightforwardly as the previous two points. Consequently, the remainder of this section will be spent in discussion of this.

4.2.2 The Individual as the Primary Epistemic Subject

This sub-section is focused on both (i) getting clear on what it means to be a "primary epistemic subject", and (ii) arguing that the individual is not the primary epistemic subject in science. To fulfil both aims, I set out three different ways of categorising beliefs in the scientific community and address each in turn to argue that these can best be categorised as fitting into a collectivist and not an individualistic framework.

When looking at beliefs in the scientific community, we can ask several different questions that relate to different ways in which we can approach this problem, and categorise these accordingly. I take it that the answer to whether or not the "primary epistemic subject" — i.e. the subject of Antony's third criterion — is best seen as collective or individual can best be decided on what fits into these categories better. If the primary epistemic subject is the collective, then it must be able to give coherent answers to the questions that arise from said categories. The categories and questions are below:

(i) Genealogy: How do beliefs in a scientific community *arise*?

(ii) Ontology: How, and in what way, do beliefs within a community *exist*?

I will examine (ii) first. Despite the name — ontology — I am not concerned with metaphysical questions of the true nature of the beliefs, or whether or not the beliefs really do objectively exist. Beliefs are taken to be real in some sense, but the question of their actual, objective and mind-independent, existence are not of concern. A helpful analogy can be seen in the way that philosophers often talk about the ontology of a theory in science; the language of an ontology of a theory will often be invoked, and yet there is not necessarily a metaphysical commitment to the ontology of this theory actually being the actual furniture of the world.

Ontology

There are many different scientific communities, which can be subdivided and also grouped together in a large variety of ways. The physics community, the string-theory community, the zoology community, the biology community, the quantum chemistry community, etc. Weatherall and Gilbert (2016) point out that when talking about these communities it is the norm to make collective statements such as 'physicists believe that elementary particles obey the laws of quantum mechanics' (p191), or that 'biologists think the chimpanzee and bonobo share a recent common ancestor' (ibid). Many other examples can be given of this. In terms of how we can think about how these beliefs in a community exist, there are two ways that are discussed in the social epistemology literature. The first is the "summative" account, and the second is the "collective" account. In the summative account, a group of scientists would be taken to hold to a particular belief if and only if all or nearly all hold this belief. This is proposed most famously by Quinton (1975-1976). The collective account, which is first proposed by Gilbert (1987), argues that this need not be the case. On this view, it is neither necessary nor sufficient that all or most scientists believe x in order to say that a scientific community believes x.

She proposes a two-part formulation:

'(i) A group G believes that p if and only if the members of G jointly accept that p.

(ii) Members of a group G jointly accept that p if and only if it is common knowledge in G that the individual members of G have only expressed a conditional commitment jointly to accept that p together with the other members of G'.

(Gilbert, 1987, p195).

On this account it is thus neither necessary nor sufficient for all or most members of the community to hold the belief in question. Many others have taken up this collective account. To name a few: Bird (2010) endorses it but argues that Gilbert and others don't go far enough in that they allegedly still posit some sort of supervenience between the social epistemic states and the individual epistemic states (2010, p24). Wray (2001) endorses it but argues instead that attitudes that groups adopt fit better into the category of "acceptance" as opposed to beliefs. Weatherall and Gilbert (2016) apply Gilbert's framework to the string theory community to account for some of the beliefs that exist there. For the purpose of this project, there is no need to choose between these, but only to recognise that they all highlight a very important fact about the unsuitability of the summative, reductionist account. The literature of collective beliefs routinely point to certain situations which are enough to rebuke these views empirically. For example, Bird (2010) argues against both the necessity and sufficiency of the claim that *there is a group knowledge iff all or most members of the group know x*. He points out, against the necessary condition, that there is no expectation whatsoever that all or even most scientists would need to know every latest finding in specific scientific journals, yet we are happy to call what is published there "scientific knowledge" (2010, p27). And against the sufficient condition, every member of a jury may have very strong opinions on the guilt of the defendant in the case in hand, but it is not said that the court finds the defendant guilty or innocent until after the court has finished (p28). It seems impossible to square these very basic cases with the summative/reductionist view.

Genealogy

The genealogical component asks the question: how do beliefs arise within the scientific community? *Prima facie*, this seems to be answered better by the individualist account, whereby the answer is that beliefs in the scientific community arise through the interaction of individuals in communication with each other and with the scientific community more generally. Then, the belief is either accepted or rejected at the level of the scientific community. This seems true, but is only part of the picture. There is also a feedback loop, and thus some significant reciprocity between the group and the individual. Once the belief is accepted as a communal belief, the belief becomes *the* belief of the community, and then begins to force itself onto particular scientists/particular members of the community.

There are a huge variety of beliefs that exist within the scientific community, with varying degrees of optionality that the individual scientist may opt in for. But it is undeniable that the beliefs that are accepted as the beliefs of a particular scientific community, in some way and to some degree, co-erce the individual scientist into believing them. This is not to say that the individual beliefs of scientists are homogenous, but simply that there is some degree of pressure to conform to the consensus.

When asking the question: *how do beliefs arise within the scientific community?*, we should thus answer that they are formed through both the individual and the collective.

An objection can be levelled with respect to beliefs in scientific communities: that it is through the individual, as primary epistemic subject, that we investigate all of this and explore all of this topic. For example, Schindler's (forthcoming) extensive empirical survey investigates the attitudes of theoretical virtues in scientists. And this does genuinely seem to be a good way of getting at what it is that scientific communities actually do believe.

But this could only ever hope to attain something like a pragmatic solution, whereby the methodology of asking individual scientists what they believe functions as practically useful but insignificant to the ontological question asked earlier. In other words, the fact that it is individuals that are being asked does not bear any significance to the way in which the beliefs exist, and should be viewed instrumentally as a tool to discern that the particular collective belief is.

Two examples can demonstrate this point well. (1) Within the particle physics community the collective belief is that the statistical significance to declare something a "discovery" it must have a p-value of equal to or greater than 5σ. The easiest way to discover this collective belief is to ask several individual particle physicists what the community believes the statistical significance should be. Practicing scientists would presumably all know the answer to this, given that it is vital to their everyday activities. You may very well get a different answer, though, if you ask the scientist what *they*, personally, believe it *ought* to be. (2) The collective belief within the physics community is that general relativity is correct. Ask any scientist what the physics community believes regarding the correctness of general relativity, and they will presumably give you this answer. Ask the individual scientist what they themselves believe, and you may very well get a different answer.

It has been shown that epistemic individualism is not compatible with a working model of science. Thus, any suitable empiricism must disregard epistemic individualism and embrace a collectivist account in this respect.

Epistemic individualism is a crucial component of EE. There is nothing explicit in a basic formulation of ME that discusses this — if a position is held here by those in the ME tradition it tends to be the absence of a positive endorsement of focusing the epistemic subject as individual. However, it certainly exists in the spirit of ME. ME prioritises scientific investigation into the world, and if proponents of ME discuss epistemology they discuss it with reference to what science tells us. Given that scientific investigation, in practice, is so clearly collective, it would be hard to find an advocate of this version of empiricism that would reject this collective component.

4.3 Practice-Led, not Theory-Led

(2) Any empiricism suitable for science should prioritise practice over theories. This means prioritising both scientific practice over scientific theories in isolation and more generally what works in practice over what works in theory.

This is straightforwardly derived from the practice-led characteristic of ME. And, as was shown in (1.1.2), the practice-led characteristic falls out of the characteristic of empirical inquiry very naturally — if the stress and focus is place onto empirically investigating the world, then it follows that we should turn to what actually happens in the world and onto scientific practice as it happens, as opposed to considering theories taken in isolation.

There is an important point of clarity that needs to be made here. The differences between ME and EE that I have characterised have a two-part component when it comes to theory-led and practice-led. The first part is that ME focuses on scientific practice rather than on theories in isolation in science, whereas EE tends to do the opposite. The second part is that ME is based on what works in practice rather than what works in theory, whereas EE focuses more on the latter.[3] Crucially: in scientific practice, scientists do use theories all the time. Thus, any sufficient account of scientific practice must not eradicate focus of theories altogether. By insisting on this claim I am therefore not proposing to do away with the study of theories, but insist instead that theories should not be studied in isolation but in conjunction with the plethora of other relations that exist in scientific practice.

When insisting that empiricism should be focused on what works in practice, rather than what works in theory, this is more of a general meta claim than one that is specifically oriented towards science. The claim will be that it is un-empiricist in essence to focus on what works in theory.

There has undoubtedly been a shift in philosophy of science over the last 40 or so years, from a methodology that is focused on theory in isolation from practice to one that incorporates far more from scientific practices. The vast majority of work in philosophy of science prior to this practice turn focused almost solely on theories. If we look at the iconic arguments that existed in this time, this is straightforward to see. First, in the scientific realism

[3] For an elaboration on these two points and how they relate to each other see the end of (1.1.2)

vs anti-realism debates that thrived roughly between the 1970s and 1980s, the iconic arguments all focus around theories. These are the no-miracles argument (Putnam, 1975), the pessimistic meta-induction (Laudan, 1981), and the argument from underdetermination.[4,5]

Important debates in the history of philosophy of science of the pre-practice turn would also generally be explicitly *about* scientific theories. To name just two: a long-running debate is that of the structure *of scientific theories*, with the two main camps being the syntactic view — see Carnap (2003), Campbell (1920) and Hempel (1952) — and the semantic view — see Suppes (1962)(1967), Suppe (1967) (1974) and van Fraassen (1970)(1972)(1980). The debates around confirmation *of scientific theories* were also extremely prominent, with important contributions from Nicod (1924), Hempel (1943, 1945), Popper (2002) and a vast amount of literature on Bayesian approaches (See Sprenger and Hartmann (2020) for a good overview).

A nice summary of the general pre-practice turn that I am trying to convey comes in the opening to Hempel (1970):

'Theories, it is generally agreed, are the keys to the scientific understanding of empirical phenomena: to claim that a given kind of phenomenon is scientifically understood is tantamount to saying that science can offer a satisfactory theoretical account of it'

(Hempel, 1970, p142).

The shift to practice emerges around the 1980s, and comes mainly via two movements. The first is the "new experimentalists", and the second is the increased focus on the more practice-led components of modelling. There is of course much crossover.[6] The new experimentalists are typically associated with Ian Hacking, Allan Franklin, Peter Galison, Nancy Cartwright,

[4]This debate experience something of a resurgence in the late 1990s and 2000s with Ladyman's (1998) introduction of ontic structural realism, with much literature devoted to various ways in which this could be formulated, arguments for it, arguments against it, and specific fields that give support to this. See Ladyman (2014) for a comprehensive overview on this.

[5]The argument from underdetermination doesn't really have a single-source iconic origin that is usually pointed to that arises in this period. Duhem (1945) [originally published in 1914] and then Quine (1951) are often pointed to as originators. Importantly, though, this thesis is established before the big debates of scientific realism that occurred in the 1970s/1980s. Laudan and Leplin (1991) argue that this underdetermination argument is established by the 1940s and 50s (ibid, p449).

[6]For example, Cartwright has made prominent contributions to both fields.

and Robert Ackermann. Soler et al (2014) list these thinkers as having in common that they *oppose* four closely related components within philosophy of science:

(1) Theory-focused (ibid, p7).

(2) The view that the main aim of experiment is to test theoretical hypotheses (ibid).

(3) The reduction of experiment to observational reports and to data as something that would confirm or refute hypotheses (ibid, p8).

(4) The "spectator theory" of knowledge (ibid).

Unsurprisingly, the new experimentalists focused on the experimental part of scientific practice.

The shift to practice that arises through modelling begins in some sense in the 1950s, insofar as work is done on models in science. Hesse (1953)(1963), Patrick Suppes (1962)(1967), Fred Suppe (1967)(1974) and van Fraassen (1970)(1972)(1980) all discuss and stress the importance of models. Suppes' focus is on scientific representation, and argues that scientific representation has to be done via models rather than linguistically. Suppe and van Fraassen are the first to propose the semantic view of theories, which proposes that theories just are models, and also argue against a set-theoretic representation that Suppes proposes. However, with respect to current philosophy of science it is with Cartwright (1983) that the literature in models shifts properly to being more practiced-focus: here, models are not just ways to see theories, but are crucially important to empirically connect theories to the world.[7] Cartwright et al (1995) invert the importance of theories to models, arguing that theories are tools for the construction of models. In their (1999b), Morgan and Morrison argue for the role of *models as mediators*, and emphasize the independence of the construction and functioning of many models from theories, thus stressing the relative autonomy and importance of models.

There has also been a shift to practice in other fields internal to philosophy of science. Tal (2013) notes that more recent approaches in the philosophy of measurement have taken a distinctively practice-oriented approach, contrasted with the more definitional approaches of around pre 2000. The older

[7]Morrison and Morgan (1999a) point out that philosophical discussions around the use of models in scientific practice can be dated back to at least as far as Maxwell, Lord Kelvin and George Francis FitzGerald (1999a, p1)

approach, predominantly the conventionalists, were criticised for neglecting real science and the roles played by experimentation in actual instances of measurement standardization (Tal, 2013, p 1162).

With hindsight, and with the clarity of the work that has been done in these areas, it now seems obvious that philosophy of science should pay attention to scientific practice, in whatever form it may take. [8] What this looks like now, i.e. in the last ten to fifteen years or so, is a large uptake in philosophers of science turning to case studies of science itself, and to models, experiments and simulations rather than theory. We have also seen a shift in the specific sciences that are focused on. There has also been a rise in the focus on sciences that have a much more practice-based methodology and rely less on established theories, including climate science, economics, and migration science. Recent works exemplifying and explicitly endorsing this methodology include Antoniou (2021), Bokulich (2018), Dardashti et al (2017), Karaca (2017)(2018), Leonelli (2016), Massimi (2007), to name just a few.

4.3.1 Why Turn to Practice?

What is it about scientific practice that would warrant an insistence that empiricism pay more attention to it? We can first ask what scientific practice *is*. To give a better answer, it is more suitable to give a predominantly ostensive definition as opposed to a predominantly intensional definition, although elements from both should be used. Scientific practice constitutes what scientific communities and scientists do in their daily activities *qua* scientists. Obvious examples of scientific practice includes designing and performing experiments, analysing the data from these experiments and discussing the data with colleagues. Creating, using and discussing theories, models, and simulations. Measuring phenomena, making predictions. There is also an inherently social dimension: attending conferences, talking to colleagues about science and particular parts of science. This list is just some of the plethora of activities that constitute scientific practice, and is not exhaustive.

Three important reasons can be given as to why empiricism should embrace the turn to practice.

[8] To reiterate a point: it should also do so without detaching itself entirely away from theories, since the use of theories are also an integral part of scientific practice.

(i) Scientific practice is a hugely important part of what science is, and to neglect it is to fail to properly comprehend science. Any philosophy of science that fails to properly comprehend science by missing this large part of it is not fit for purpose.

This reason serves as a necessary condition as to why we should take scientific practice seriously. It is very minimal, and doesn't aim to convince readers of the need to prioritise scientific practice, only that it is important to pay equal attention to it. All that has been shown is that it cannot be neglected, not that it should be prioritised. The argument for prioritisation comes in (ii) and (iii). The point of suggesting this is that despite seeming like a truism, it is a historical fact that this has not been the case for a long time in the history of the philosophy of science. It is thus a minimal condition for any suitable empiricism to pay some sort of attention to it, given that it makes up a vital part of what science actually is.

What this looks like in application is that an empiricism suitable for science should not be making philosophical claims that run contra to scientific practice. One such example which will be explored in the follow section in greater depth is around observability. If scientific practice uses a notion of observability which is at odds with the way that empiricism uses it, then this is not compatible with scientific practice.

(ii) Examining what works in practice rather than what works in theory should also be in the essence of empiricism. There is something inherently unempiricist about focusing on what is theoretically the case, as opposed to what actually is the case, in experience.

This is more of a general philosophical point than it is in reference to science. As has been shown over the course of the first two chapters, theoretical, or theory-led, reasoning has played a prominent role in EE. Arguments will often be constructed that don't have much grounding in experience at all and employ a sort of reasoning that doesn't appeal to what is actually the case, nor does it have any empirical consequences that can verify this. For example, Locke's taxonomy of both ideas and powers that were highlighted in 1.4.2. The former seems to have come from Descartes (Rogers, 2007, p15), and is taken up again by Hume. The method of separating ideas into simple and complex, and then to propose that simple ideas are the fundamental building blocks of all ideas (see 1.4.2), which merge together to form complex ideas, is a prime example of this sort of theoretical reasoning that I have in mind. His taxonomy of powers into "active" and "passive" powers is similar in structure here (see 1.4.2). When we examine Berkeley's process of arriving at his conclusions, the same process is employed. This is made most clear in his *Three Dialogues*, where two characters talk to each other in Socratic fashion and proceed by a logic of purely theoretical reasoning to arrive at the conclusion that only ideas exist. The way in which the conclusion is arrivet at is entirely via abstract contemplation and reflection, and not on turning to the world itself to examine it. In more recent times, Carnap's process of reasoning in his *Aufbau* employs this sort of reasoning. The project uses "elementary experiences" as its foundation of sorts in order to rationally reconstruct science (see 2.3). There is no evidence, in practice, that there is anything akin to these elementary experiences that we find when we go out into the world to look; they seem instead to be a purely fictional construct that are posited for theoretical convenience.

The paradigmatic instance of this that I want to focus on, and is one that has arguably had most influence within philosophy of science, is Hume's problem of induction. Recall the structure of this argument is as follows, taken directly from the end of chapter one of this thesis:

(H1) All reasoning concerning matters of fact are founded on the relation of cause and effect (Hume, 2007b, p19) .

(H2) Moving from cause to effect requires an inference that nature is uniform in some sense (call this the uniformity principle (UP)).[9] Thus, inferences are founded on the UP, and cause and effect is founded on both inference and the UP.

(H3) We need an epistemic justification for the UP.

(H4) We cannot justify the UP by a priori reasoning since a priori reasoning concerns truths/statements that could not be otherwise, whereas matters of facts require a posteriori reasoning (from Hume's fork).[10]

(H5) And we cannot justify the UP by a posteriori reasoning because this is circular. For any type of a posteriori reasoning uses cause and effect, which in turn uses induction and presupposes the UP. So we are justifying the UP

[9]'all our experimental conclusions proceed upon the supposition, that the future will be conformable to the past' (2007b, p25-26).

[10]Section IV, part 1 of the *enquiry* boils down to this

by appealing to something which presupposes the UP and thus presupposes what it is trying to justify.

(HC1) Therefore the UP cannot be epistemically justified and can't be known through reason.

(HC2) The UP is a foundational assumption to induction, and therefore if the UP can't be know through reason then induction can't be known through reason.

What this is meant to show is that we don't have rational justification to make ampliative inferences. Despite this being often seen as perhaps *the* paradigmatically empiricist argument, there is something inherently un-empiricist in the structure and process of reasoning in the argument.

To get this point across with more force, I want to examine two more arguments which hold a more empirical structure, but would be hard-pressed to find philosophers who would place them in the empiricist tradition of any sort. The first is one of Zeno's paradoxes. Aristotle writes of this paradox of motion that it 'asserts the non-existence of motion on the ground that that which is in locomotion must arrive at the half-way stage before it arrives at the goal' (Aristotle, Physics, 239b11). We can set the paradox out as follows:

(Z1) For a system to go from a to b, it must first reach a halfway point, c.

(Z2) For a system to go from a to c, it must first reach a halfway point between this aforementioned halfway point, call this d.

(Z3) For a system to go from a to d, it must first reach a halfway point between this aforementioned halfway point, call this e.

(Z4) This process will go on infinitely.

(ZC) Therefore motion is impossible, since to get from any place to another place one must cross an infinite amount of points.

The second is explicitly in reference to Descartes' hypothetical argument for global scepticism that he presents in his first meditation, but is a fairly general argument for scepticism.[11]

[11]It's important to note that Descartes' presents this argument in order to defeat the sceptic by arguing against it throughout the rest of the Meditations

(D1) Everything that we have accepted as true we have acquired either from the senses or through the senses.

(D2) The senses have historically deceived us to a great degree, despite at one time us entirely believing them to be veridical.

(DC) Therefore we should not trust our senses to be veridical.

In Zeno's paradox and Descarte's argument, the premises — with the exception of Z4 — are all empirical, and it is the conclusion that makes an ampliative inference. In the case of Zeno, it makes an ampliative inference to something that is clearly empirically false; we do, as a matter of empirical fact, see things move from one place to another all the time, despite having to traverse an apparently infinite number of halfway points. In the case of Descartes, the argument is not really something that can be straightforwardly cross-checked with experince and thus cannot be said to clearly be empirically true or false.

With Hume's argument, there are at least several premises that are non-empirical, and thus appeal to some sort of theoretical reasoning. With Hume's conclusions, we arrive at a conclusion that is not so far away in its extreme claims to Zeno's when it comes to the consequences that the problem has for science. Ampliative inference is at the very core of science, and science is certainly the most rational enterprise that we currently have. We use statistical inferential procedures constantly in every form of science, and have extremely sophisticated techniques to do this. And yet if we follow Hume's argument, none of this is rationally justified. Hume's problem is asking us to place our faith in a — albeit convincing — process of highly theoretical and abstract reasoning in order to arrive at a conclusion that goes directly against how we experience the world (science being founded on irrationality). In addition to this, Hume's apparently empiricist argument is at odds with empirical scientific practice: Hume introduces a singular uniformity principle; science in fact uses many uniformity principles (Evans and Thébault, 2020)(Sober, 1988)(Okasha, 2001)(Norton, 2003)(Okasha, 2005). Evans and Thébault list, amongst others, temporal uniformity, spatial uniformity, and intra-type uniformity (2020, p4).

Here is the crux of the problem that I am pointing to: Hume's argument is less empirically focused than these two arguments that I have set out, yet is taken very seriously by many in the EE tradition and is taken to be a pinnacle of empiricism by many. It seems clear that the arguments of

Zeno and Descartes are not empiricist arguments. But how, then, is it that they seem to be more empiricist in structure than Hume's — in that they contain more premises that are directly based in experience and investigation — and yet are both rejected as un-empiricist while Hume's argument is seen as thoroughly empiricist? This is a problem for any sort of empiricism that prioritises theoretical reasoning in this way, but which an empiricist of the ME version has no problem: their solution is simply that none of these arguments are empiricist.

Against this more theoretically-led reasoning which doesn't focus on what is actually the case, or does but then draws large leaps going from the premises to the conclusions, we should prioritise what actually happens in practice. What, then, would a properly empiricist argument look like? In current times, it means using the tools of science where we can in drawing out predictions and projections. In Hume's example of the sun rising, instead of turning to theoretical, abstract reasoning, we would turn to the scientific practices of statistical inference and astrophysics. Statistical inference is an extremely sophisticated areas of statistics that has complex and well-tested procedures to obtain relatively veridical predictions, and astrophysics is a reliable science that tells us what actually happens in practice. A loose account of this would be exploring the properties of the sun as a star to try to determine it's rough life-span and then using the evidence from this to determine the likelihood of the sun being in existence and the earth continuing to orbit it in the next 24 hours.

(iii) Turning to Scientific Practice is in the Essence of Empiricism. Looking to case-studies in science, both current and historically, and dealing with them should be at the heart of empiricism.

I take it for granted here that one should be turning to science rather than doing abstract, a priori theorizing of what is the case. This is presupposed and not argued for. I am here proposing that one should be specifically turning to scientific practice, and to *what is actually the case* in science, as opposed to focusing just on scientific theories in isolation.

Crucially, this is the case because theories present us with idealized and abstract rules that do not actually reflect what happens in experience without qualifications and various elements of scientific practice being able to apply theories to reality. They require parts of scientific practice in order to be able to say what is actually the case, or to make predictions that do reflect reality.

As Winsberg (1999) puts it: '[b]y itself, the theory tells us very little about anything but the most idealized system. To apply them to the real world systems require a mechanical model' (Winsberg, 1999, p7). I take this to be an uncontroversial point, that even those who focus exclusively on theories have at least partially acknowledged.

The received view/syntactic account/conventional view of theories, depending on which terminology you use, typically accounts for the connection between theory and empirical reality via what are termed "correspondence rules" (Nagel, 1961)(Carnap, 1966) or "bridge principles" (Hempel, 1966)(Hempel, 1970).[12] The basic structure of a scientific theory is that of "internal principles" and "bridge principles". The former detail the entities, processes and laws postulated by the theory, and the latter link the theory to the observable phenomena (Hempel, 1970). The focus of these principles are solely around converting so-called theoretical terms to what is observable via sensory perception, whilst taking for granted that such things will hold in all instances.

The semantic conception of theories arises as an alternative to the syntactic conception, and is mainly formulated either via set theory (Suppe, 1977) or state-space (van Fraassen, 1980)(van Fraassen, 1989). There have been a few different ways proposed of connecting theories to empirical reality under this approach, but the most popular seems to be that of a notion of isomorphism between the models and reality. This is the approach that has been taken up by, amongst others, van Fraassen (1980), French (2017), and Ladyman (2004).[13]

Perhaps the most in-depth discussion of the relation of theories to the world is through Cartwright, who is deeply critical of the above two conceptions. The connection between what theories *say* and what *actually happens* is a central part of the latter half of Cartwright's HTLPL (1983, p100-164), and also crops in various other places.[14] She criticises these traditional accounts of bridge principles for being far too idealized and not corresponding to how we would actually apply the theory to reality in scientific practice. For instance,

[12]Winther (2020) lists several other terms in which this idea has been conveyed in the syntactic view including "operational rules" (Bridgman, 1927), "co-ordinative definitions" (Reichenbach, 1969), "reduction sentences" (Carnap, 1936/1937), (Hempel, 1952), "correspondence postulates" (Carnap, 1963), "reduction functions" (Schaffner, 1969), and "bridge laws" (Sarkar, 1998).

[13]And several co-authored papers between French and Ladyman, including (1997, 1999, 2003).

[14]See (Cartwright, 1999, p35-49), (Cartwright et al, 1995, p140).

in QM there does seem to be something that corresponds to the structure of internal principles and bridging principles. We have the Schrödinger equation serving as the central internal principle, and observable quantities are represented by operators (ibid, p135). This is apparently necessary and sufficient for characterising a theory according to the syntactic view, but Cartwright's point is that one can understand this framework absolutely perfectly and yet still have no real idea of how to apply QM to the actual world. To apply QM to the real world, we have to know how to choose the right Hamiltonian (ibid), and this is learnt through direct practice and usage. The theory alone, regardless of it having bridging principles, cannot connect to reality and tell us what will happen in concrete situations without specific models that we specifically select for these circumstances. A model 'is employed whenever a mathematical theory is applied to reality' (ibid, p158). Cartwright writes:

'To have a theory of the ruby laser, or of bonding in a benzene molecule, one must have models of these phenomena which tie them to descriptions in the mathematical theory.'

(ibid, p159).

Cartwright is keen to differentiate this fact from the semantic view as well as the syntactic view, and contrasts her views with that of van Fraassen's mainly around the fact that van Fraassen focuses on observable structure , and that for van Fraassen the unobservable content need not correspond to reality. Cartwright wants to focus on what is the case, on 'what actually happens in concrete situations' (ibid, p160), and this includes all levels of reality, not just what is observable for humans.

As can be seen, Cartwright's stress here is heavily on models, and how models should be the focus of philosophy of science more than theories. Cartwright endorses a radical view about the nature of models, and the relation between models and theories that has evolved over time (see (Bailer-Jones, 2008)). This need not be endorsed to convey the point that she is trying to get across here. This is not a point that is unique to Cartwright. It is simply a fact that theory *by itself* cannot give us clear empirical predictions and thus connect us to the world due to the more abstract and general nature in which theories are formulated. Theories are incredibly useful in how general they typically are, but without sophisticated techniques that we have in scientific practice, we cannot connect theory to the world and thus learn anything empirical. Winsberg (1999) expands on this somewhat. As stated above, a mechanical model is needed to be able to start applying theory to the world — this gives it a 'bare bones characterization' (ibid, p7) of a real physical system. But the mechanical model is still too general to be able to be about any *particular* system. Parameters, boundary values, and initial data are required to make a dynamical model which we can then apply to a specific system (ibid, p8). Whilst philosophers such as Gähde (2008) have criticised Cartwright's approach here, the criticism has been on the specific details in the methodology that she advocates implementing to move from theory to reality, and not on the fact itself that theories alone cannot tell us about the world.

Another instance of this theoretical-practical relationship is the relationship between fundamental laws and phenomenological laws. The former are theoretical, and give grand generalised accounts of how phenomena will allegedly behave. The latter are descriptive, and give specific accounts of how phenomena actually do behave in very specific instances. The typical account, which Cartwright calls the 'generic-specific' account (1983, p103) explains this fact by arguing that phenomenological laws are applied instances of general laws, and can be derived from them. She argues, though, that there are a plethora of examples where this is simply not the case, and can be split into two camps — (1) instances where phenomenological laws are more accurate than the derivations that we would get from the fundamental laws (ibid, p106). She gives examples of an amplifier (ibid, p107-112) and the exponential decay law (ibid, p113-118) to assert this. And (2): instances where phenemenological laws are supposedly just applied instances of fundamental laws, seldom are the facts enough to justify this derivations (ibid, p106). She gives examples of the lamb shift in both ground and excited states to support this claim (ibid, p119-126).

Laymon (1989) argues that Cartwright fails, in her prioritisation of phenomenological laws over fundamental laws, 'to take into account the relevance of the piecemeal improvability of idealizations and approximations and the corresponding improvements in predictive output' (Laymon, 1989, p353). But Laymon unreservedly acknowledges — and argues that any holder of the generic-specific account will have to also acknowledge — that this view that the generic-specific account is not descriptive of actual science (ibid, p355). Instead, they must give their account a normative reading, whereby the 'goal of science should be to seek fundamental laws which are true, and can be used in the sound derivation of phenomenological laws' (ibid). But this is entirely at odds with an empiricist project that seeks to emphasise scientific practice, and to examine what is actually the case, in practice. It is evident that this has not been the case, as Cartwright has demonstrated.

And to impose a normative aim onto science of finding fundamental laws that can lead to the derivation of phenomenological, empirical laws that already work very successfully seems extremely regressive.

There is a stronger claim that Cartwright makes, and that certainly is at the heart of her empiricism, which I want to remain silent on. That is the slightly more radical view that results in the patchwork view of nature, where, for example, classical physics is said not to be properly replaced by quantum physics because quantum physics works well only in specific situations, and has not performed nearly as well in many instances where classical physics works very well (Cartwright, 1999, p2).

The key point of this explication that I want to stress is the fact that theories, taken alone, cannot tell us about what actually happens. Introducing Cartwright's arguments here have been done in order to try to make this clear, and not to specifically promote Cartwright's positive claims. The fact that is of vital importance is that to *use* a theory in science, in whatever way we want to use it, we cannot just take the theory alone — we need elements of scientific practice to be able to do this.

4.3.2 Van Fraassen

It may be objected that van Fraassen's brand of EE *does*, in fact, pay close attention to scientific practice. Case studies are drawn upon frequently through his work, and it is clear from reading any of his work in philosophy of science that he knows and engages with the relevant material.[15] I argued in the previous section why I believe van Fraassen to be far more in the camp of "theory-led" as opposed to "practice-led" in terms of the criterion in EE vs ME. I argued here that the central focus of constructive empiricism isn't about going into the world and investigating, but is about both theory acceptance and trying to understand what we can know through theories, and that constructive empiricism — as van Fraassen formulates it — is completely tied to a voluntarist foundation. Constructive empiricism, combined with voluntarism, is what I argue to be theory-led. Whilst more recent work by van Fraassen, including especially his (2008), do focus greatly on scientific

[15]For just a few examples of these case studies, see his example of measuring the charge of the electron in (1980, p74-77); his three examples of paradigmatic scientific explanation (1980, p101-103); and perhaps most relevant here is his book-length treatment of treating quantum mechanics (1991). His (2008) also pays great attention to scientific practice.

practice, he is nonetheless still committed to a position of voluntarism and constructive empiricism, which tie together in this aforementioned way. It is this position that this thesis focuses on, and this position that defines van Fraassen's particular brand of empiricism.

But one may also point to the fact that van Fraassen holds as central, in his broader conception of empiricism, that scientific inquiry is a paradigm of rationality. Keeping in mind that his constructive empiricism must, in virtue of being a special instance of empiricism, take on board the key features of empiricism, one may *prima facie* reasonably assert that van Fraassen does place great stress on scientific inquiry, and on scientific practice. But once we take into his account his voluntarism — which, as has been shown in the previous section, plays a necessarily foundational role for his constructive empiricism — then there is a tension at the heart of his position which largely throws into doubt what this commitment to scientific inquiry/scientific practice can actually look like. The tension revolves around his claim that part of being an empiricist is to have the attitude that scientific inquiry is a rational form of inquiry par excellence, and yet also holding, qua voluntarist, that it is perfectly rational to not believe in science. Summarised:

(1) Van Fraassen says that part of being an empiricist is to have the attitude that science is a paradigm of rational form of inquiry, and that science is the rational form of inquiry par excellence (2002, p63).

(2) Van Fraassen's voluntarism, via its permissive conception of rationality, says that you can refuse to believe in science and be rational.

(3) Normally, the empiricist-voluntarist can escape tension between the permissiveness of voluntarism and the definitiveness of empricism (or most positions) holding certain beliefs by saying that to not believe x does not make one irrational but just makes one not an empiricist. But there is a tension in holding both (1) and (2) simultaneously due to both making claims about rationality, of sorts.

(4) The tension can be stated thus: van Fraassen's empiricist-voluntarist claims, qua empiricist, that science is the rational form of inquiry par excellence and is a paradigm of rational inquiry. This seems to imply that to not believe in the rationality of scientific inquiry is to *not* be rational. Yet the empiricist-voluntarist also claims, qua voluntarist, that one is free to not believe in scientific inquiry and also be rational.

Points (1) and (2) are hopefully apparent from the above section of van

Fraassen's philosophy and it is assumed that they are straightforward and do not need clarification. This exposition thus focuses on (3) and (4).

Voluntarism is a very broad meta-position that governs and dictates terms to subordinate positions in some sense. Specifically, it dictates (extremely minimal) terms regarding what one can believe and how these beliefs hang together if one wants to be rational, and is both highly permissory rather than obligatory and has minor conditions of what you shouldn't believe, on pains of irrationality. The *empiricist*-voluntarist, qua voluntarist, must therefore believe that the only rule that requires universal assent, with respect to beliefs, is the aforementioned minimal constraints. The empiricist can thus believe that, say, certain metaphysical inquiry should be rejected, but accepts that this is a condition of what it is to be an empiricist, and would not assert that to not believe this is to be irrational. In other words — to be an empiricist is to reject certain forms of metaphysical inquiry, but to not reject metaphysical inquiry is not to be irrational. Instead, it would just eliminate you from being an empiricist. This can be phrased in a sort of meta-claim about how voluntarism and other more specific positions which hold to a voluntarist foundation interact:

It is compatible to hold to a voluntarist theory of rationality and ascribe to any more localized, specific position X that prescribes you hold certain beliefs in virtue of believing in/ascribing to X, since the beliefs that you would hold to and prescribe qua holding X would not be you demanding universal assent to the beliefs, but only localized assent.

One can believe anything they want to believe — providing aforementioned constraints — under voluntarism, and ascribe certain prescriptive beliefs to any more specific position so long as the more specific position does not declare that to not believe it is to be irrational.[16,17]

[16]We can see this point made in more abstract form in his (1989), when he talks about accepting constraints upon beliefs and belief systems in voluntarism (p320-321).

[17]Van Fraassen speaks on a few occassions of not being able to straightforwardly equivocate attitudes and beliefs. See for instance (2002, p47)(ibid, p62). But there are clear instances where he does clarify that attitudes and stances *do* require and presuppose some beliefs. Three instances of this will suffice:

'[the attitudes] may well involve or require certain beliefs for their own coherence'
(ibid, p47)

'Such a stance can of course be expressed, and may involve or presuppose some beliefs as well'
(ibid, p48).

'Stances do involve beliefs and are indeed inconceivable in separation from beliefs and opinion. The important point is simply that a stance will involve a good deal more, will

Now to approach the problem. The tension that is referred to in (3) exists because van Fraassen's empiricist-voluntarist makes a claim involving rationality *as an empiricist*: that science is the paradigm of rational inquiry/that science is the rational form of inquiry par excellence. Yet they must still hold the voluntarist view that it is potentially rational to not ascribe to this specifically empiricist view on what is rational. Whilst what the empiricist has said is clearly not an exhaustive account of what is rational, and neither have they tried to give conditions for rationality that the voluntarist position gives, the position sits awkwardly. The situation is the following: *on the one hand*, qua voluntarist, they have a very relaxed criteria of rationality whereby all they need to do to be rational is abide by a few aforementioned minimal constraints. On the other hand, *qua empiricist*, they believe that science is an exemplar of rational inquiry; this seems to imply that to be rational involves adhering to the view that scientific inquiry is the rational form of inquiry par excellence. To expand on this point more, let's imagine two brief exchanges.

Person 1: I believe that speculative forms of metaphysics, and metaphysical inquiry more generally, are viable ways of doing philosophy.

Empiricist-voluntarist: Fine. You are definitely not an empiricist, but I do not think that you are not rational.

Contrasted with:

Person 2: I reject that science is the rational form of inquiry par excellence and I don't believe in science; either in the process of scientific inquiry or in the products of science.[18]

Empiricist-voluntarist: Fine. You are definitely not an empiricist, but I do not think that you are not rational.

Whilst the first conversation seems perfectly adequate in terms of coherency, the same cannot really be said of the second conversation. To hold that some discipline is the rational form of inquiry par excellence, and yet simultaneously hold that one can entirely reject this same discipline and that this doesn't make them not rational, seems to miss the point somewhat. If the empiricist-voluntarist, qua empiricist, can say that something is the rational

not be identifiable through the beliefs involved, and can persist through changes of belief' (ibid, p62).

[18] Person's 1's views are formulated in positive terms and Person 2's views are formulated in negative terms, but this makes no difference to the logical structure of the argument.

form of inquiry par excellence, yet never actually say that to not believe it must consequently lead to not being rational, then it seems that claims about scientific inquiry being exemplars of rational inquiry are effectively useless. Thus, regardless of how strongly van Fraassen wants to hold to scientific inquiry and practice as being an exemplar of rationality, it is overridden by this tension at the core of his positions.

4.4 Measurement, not Observability

*(3) Any empiricist position in current philosophy of science should not give the greatest epistemic privilege to **observation**, but to the initial stage of the **measurement** process.*

This criteria can be derived from a combination of *every* characteristic of ME. If we go out and **empirically investigate** the world in such a way that **de-centralises sensory perception**, then measurement is the natural place to turn to to. Insofar as performing measurements will often measure phenomena that are unobservable via sensory perception, and Cartwright claims that we can measure causes and capacities (Cartwright, 1989), then there is a **metaphysics** of some variety. And insofar as what I will go on to propose is to place epistemic privilege upon the initial stage of measurement, as opposed to the entire process; and to doubt that sensory perception or even observation is the best guide to what we should epistemically privilege, there is a **mild scepticism at play**. In discussing what should or should not be most epistemically privileged, there is a clear epistemological focus. But this epistemological query is decided not by turning to philosophy itself, but by turning to science, thus there is simultaneously both a **shift away from philosophy-first epistemology** and a **practice-led** approach at work here.

Further, other criteria that have been given thus far lend great support to this claim. If we are to focus on the collective rather than the individual more generally, and to turn to scientific practice, we find instantly that science is simply not concerned with sensory perception, but is far more concerned with the collective process of measurement.

Often within EE, and various other anti-realist positions, the demarcation of what is observable and unobservable in science has been drawn at human sensory perception. Special epistemic, or even ontological, privileges are then given solely to observable phenomena. Belief in unobservable entities is sometimes withheld on the basis that they are unobservable. Here, I argue that this is entirely unsuitable for an empiricism compatible with a modern science, where huge amounts of science are primarily concerned with entities and structures that are unobservable.

There is a balancing act here between two criteria — one *restrictive* and one *permissive*. We want to both remain empiricists and specifically in the ME tradition by focusing exclusively on empirical investigation and evidence (restrictive criterion), and yet also allow for anything that counts as empirical evidence in some important respect in the ever-evolving domain of science (permissive criterion). I argue here that the best way to do this is by focusing on *measurement*, and not on observations. Specifically, the first stage of a measurement, where the measuring device interacts with the system and takes an initial reading. Measurements both retain the restrictive empirical character that empiricists desire — we are going out into the world and measuring phenomena — and yet are suitably permissive to be a criterion by which something is considered to be empirical or not. This is similar claim to that of William Thomson, who I documented in (2.1.2) as being in the ME tradition.

'I often say that when you can measure what you are speaking about, and express it in numbers, you know something about it; but when you cannot measure it, when you cannot express it in numbers, your knowledge is of a meagre and unsatisfactory kind'

(Thomson, 1891, p73)

The structure of this section is as follows. First, I examine traditional motivations for drawing the observable/unobservable line at the level of sensory perception. Next, I set out arguments from Musgrave and Ladyman and generalise them further to show that drawing the observable/unobservable line in this way is internally incoherent as commonly practised, and can be made coherent if one is either willing to endorse certain unobservable structures in the form of counter-factuals, practically speaking, in current science. Then I examine hypothetical attempts to simply extend the line further, yet retaining the sensory component via treating scientific instruments as being extensions of our sensory organs. I argue that this fails, primarily because it is not permissive enough. Finally, I make a positive claim that measurement should be the preferred option. Specifically, I argue that it is the first stage of measurement that we should give greatest epistemic privilege to.

4.4.1 Epistemic Privilege

When I speak of "privileging", and specifically in this context of "epistemically privileging", this should be understood not as a binary between privileged and non-privileged, but as a spectrum. If one is to epistemically privilege x then one holds x in some high regard with regards to how justified one thinks this belief is. There are degrees of epistemic privilege that one can give, including relational terms between several different classes. We can differentiate between absolute and relational epistemic privilege, also:

absolute: x is what we should epistemically privilege over everything else

relational: we should epistemically privilege x more than y

The focus here is around the claim that forms of EE give *greatest*, or some sort of absolute, epistemic privilege to sense perception. I claim that we need to move away from this, and that the *greatest*, or some sort of absolute epistemic privilege, should be given to measurement. Specifically, the first stage of the measurement process, prior to all the various inferential and analytic procedures begin that require going from a "raw" measurement to a final output.

A clarification should be added: the degree of epistemic privilege that should be awarded to sensory perception has changed over time, and sensory perception certainly held greater epistemic weight than it does now for a long time in human history. Before the advent of scientific instruments, including more sophisticated measuring devices, the only sort of measurements we could make were with naked sensory perception. When this was the case, sensory perception should indeed have had great epistemic privilege. But this was a time that was prior to even many of the Early Modern empiricists in the EE tradition who were stressing sensory experience as so important. At the time of Locke's writing, one of the earlier figures in the Early Modern EE tradition, Boyle was already performing experiments that went beyond sensory perception; and Locke was fully aware of these experiments. My point is this: once, a long time ago, human senses were the best way of measuring phenomena, and thus at one time in history they were rightly given great epistemic privilege. But this day has long passed.

4.4.2 Motivations for the observable/unobservable line being drawn at sense perception

Traditionally, the argument for drawing the line at the level of human sensory perception, and prioritising the observable, has been via an appeal to sense experience as being more epistemically secure than non-sensory experience. The earliest example we can see of this is the empiric school from the Hellenistic period, as briefly discussed in chapter one. They were medics, and refused to posit causes and entities beyond what can be directly observed on the basis that this type of knowledge was more open to scepticism that what we can observe directly. The Early Modern empiricists of the EE version all held to this in some form.[19] The logical positivists also adhered to this, all drawing the observable/unobservable line around sensory perception and giving some form of privilege to the latter. Carnap and Schlick both attempted to develop secure epistemologies around sensory experience at the core; Neurath does not concern himself with epistemology but holds to the distinction and privileging of sensory perception nonetheless. Thus we see in them, and other empiricists such as Mach and Mill (see chapter two), a formulation of empiricism around sensory perception and aiming to stay clear of a metaphysical commitment to realism.

Whilst there is certainly something of this in van Fraassen's brand of empiricism, it doesn't seem to be the chief motivation behind it. If it was, he would presumably go for the most epistemically secure option; but he explicitly acknowledges that there is risk involved in believing in a theory as empirically adequate, and that 'we stick our necks out' (van Fraassen, 1980, p69) when we believe in empirical adequacy. This is because 'empirical adequacy goes far beyond what we can know at any given time' (ibid), for we must believe in not just all actually observed phenomena, but all potentially observable phenomena (i.e. in the future, in different spatial locations, etc). Van Fraassen also explicitly does believe in the reality of the everyday world, and intends to formulate his constructive empiricism around this. There is involved in this an explicit rejection of sense-data. He writes:

'I wish merely to be agnostic about the existence of unobservable aspects of the world described by science — but sense-data, I am sure, do not exist'

(ibid, p72).

If your chief priority is epistemic security, then it seems like a logical consequence of this that sense-data would be the *most* secure — we can doubt in the reality of the table, the chair, or the cup, but we cannot doubt that we have an experience of the table, the chair, or the cup, according to sense-data advocates.[20] It seems therefore that van Fraassen is looking for a middle ground between absolute epistemic security and the type of epistemic security that is compatible with science. His voluntarism surely plays an important

[19] There, the contrast is with speculation or a priori judgement.

[20] Van Fraassen doesn't really expand on why he holds his position of rejecting sense-data outright. But one can assume that he takes Sellars' "myth of the given" seriously, which is an argument designed to show the impossibility of holding a foundationalist epistemology with sense-data as the foundation. Sellars is clearly a hugely influential figure in van Fraassen's philosophy.

role here, too. Under voluntarism, he is perfectly licensed to draw such a distinction, and advocate for belief only in the empirical contents of the theory. Van Fraassen also makes clear that for an empiricist position more generally, it is not essential that one draw the line where he does, only that one draw one more generally and define it (van Fraassen, 2008, p110)

To an extent, criticising van Fraassen for the reason that there is not good positive justification for drawing a line where he draws it is to miss the point of his project. Motivated by the voluntarist foundation, van Fraassen does not require positive justification to make these claims; he requires only that the position meets the very minimal criteria of rationality that has been set out above. But this does not concern this project at hand — the goal here is to try and explore what a position in ME looks like with respect to current science, and to argue also for this position's success. Thus, this project most definitely *is* trying to positively justify an empiricist position, rather than simply trying to show that it's not irrational. The voluntarist foundation is not endorsed.

4.4.3 Empiricism Must Accept "Unobservability"

As Musgrave (1985) and Ladyman (2000) essentially show, for an empiricism within science to be at all suitable some version of unobservablity must be accepted. First, it is helpful to introduce some distinctions that Churchland (1985, p39) makes and comments on between different types of unobservability. He lists six in total, and argues that the list could be extended. Listing 4 of these suffices for present purposes. (i) Unobservable because they are too far away in space and time; (ii) unobservable because they are too big or too small, (iii) unobservable because they don't have the appropriate energy to be detected by our sensory apparatus, (iv) or unobservable because they aren't subject to fundamental forces, amongst others. Churchland points out that despite all being unobservable in some important sense, (i) is treated as "observable" in common philosophical usage.

Musgrave's objection to the observable/unobservable distinction drawn by van Fraassen's constructive empiricism is extremely brief, spelt out over just one paragraph (Musgrave, 1985, p207-208). He highlights two points that van Fraassen makes which he claims are internally incoherent. The first is that what is or is not observable or unobservable is determined by human physiological limitations. The second is that, as van Fraassen acknowledges, these physiological limitations are known to us via science. Imagine, Musgrave says, a theory that definitively tells us these limitations. Clearly the theory will involve both observables and unobservables, in virtue of the fact that it demarcates between the two. It will tell us that A is observable for humans, whilst B is not. But the constructive empiricist can only accept this theory as empirically adequate, and thus cannot take the limitations given by this hypothetical theory as true, only empirically adequate. Which clearly in this case is not the whole picture – it is not true – because it says something definitive about the unobservable.

Ladyman's (2000) interpretation of van Fraassen differs. He reads van Fraassen as claiming that what is observable/unobservable is an empirical fact, and is theory-independent (Ladyman, 2000, p850). This would avoid Musgrave's problem. Ladyman insists though that constructive empiricism must both (i) commit to some sort of theory-dependence to demarcate what is observable and unobservable, and (ii) commit to belief in at least some counterfactuals (ibid, p851). (i) Arises because the potential observation of certain phenomena, for example, dinosaurs, is clearly known theoretically; (ii) arises beacuse of the fact that there is a counter-factual claim at play here — *if* I were to exist in the jurassic period, *then* I would be able to observe a dinosaur/*if* I were to travel closer to Jupiter, *then* I would be able to directly observe Jupiter. Ladyman thus argues that van Fraassen must 'abandon either constructive empiricism or his modal antirealism' (ibid, p852).

There is also a larger problem that Ladyman here points to: that even if these internal tensions weren't fatal, *science requires modalities*. We look for theories of the observable, not the already-observed (ibid). Ladyman's point here can be generalized to all positions that draw this distinction and not just constructive empiricism: to classify a thing that is observable-in-principle yet not practically observable necessarily requires a counter-factual which those who advocate for a rejection of unobservable entities and structure cannot facilitate, since they reject knowledge of such unobservable structure. For a philosophical position to coherently hold to any sort of observable/unobservable distinction, the feasibility of this depends on whether or not this philosophical position is willing to adopt unobservable structures, namely modality in the form of counter-factuals. We thus have an ultimatum:

If one *is* willing to adopt this unobservable structure, then one can draw the line wherever one wishes and remain internally coherent. If one is *not* willing to adopt this unobservable structure, then one must remove the class of "observable-in-principle" that Churchland draws out above (e.g. dinosaurs, the moons of Jupiter, and any event that has not been actually directly observed).

Whilst either option is internally coherent, once examined with regards to their external application, the latter is evidently not suitable for any sort of working science that we currently have. Trying to formulate a philosophy of science that deals only with events and processes that have actually been observed, without generalising to future events or events that we don't directly observe, is so alien from science that it renders this formulation wildly unfit for purpose. The former, at least *prima facie*, applies perfectly adequately to science. The sort of counter-factual reasoning so crucial to science is preserved, and the observable-unobservable distinction can be saved in this manner.

4.4.4 Practical Considerations

The previous subsection showed, via Ladyman (2000), that it is theoretically *possible* to draw such a line of observable/unobservable *only if* one is willing to introduce unobservable structures via counterfactuals. This section shows that it is not feasible to do so if one then goes on to give any sort of privilege to the class of phenomena that are observable with the naked eye. It is thus focused on practical considerations. These practical points are taken into consideration, naturally, because of the aforementioned point that any suitable empiricism for science should be grounded firmly in scientific practice. But there are also good reasons in and of itself that practice should be taken into account here.

Hacking (1985) argues against the drawing of the observable/unobservable line at the level of sensory perception by turning to case studies concerning the microscope. The argument is that we *do* see through a microscope, or at least through microscopes as they have existed in more modern times. He presents something of an "argument from coincidence" via a case study of how we go about seeing a red blood cell. We can use two microscopes that use very different procedures to "see" – an electron microscope and a fluorescent microscope – and both will confirm that there are dense bodies in red blood cells (Hacking, 1985, p144). Hacking is quick to point out that this should not be seen as an argument from coincidence in the same style as Smart (1968) or others endorsing the no-miracles argument or general "inference to the best explanation" to unobservable entities/ a position of scientific realism. Hacking's argument is intended to be far more localized:

'With the microscope we know there are dots on the micrograph. The question is, are they artefacts of the physical system or are they structures present in the specimen itself? My argument from coincidence says simply that it would be a preposterous coincidence if two totally different kinds of physical systems were to produce exactly the same arrangements of dots on micrographs'

(Hacking, 1985, p146)

The following analogy nicely expresses Hacking's view towards privileging sensory perception:

'it is doubtless of some small interest to know the limits of the naked eye, just as it is a challenge to climb a rock face without pitons or Everest without oxygen. But if you care chiefly to get to the top you will use all the tools that are handy'.

(Hacking, 1985).

Shapere (1982) turns to how scientists themselves formulate what is observable and what is not. Shapere proposes an observable/unobservable distinction that is motivated directly by physics, using a case study of scientific practice with respect to the "observation" of solar neutrinos. Picking up that philosophers understand the term "observable" very different to how scientists use it, he points to an important contrast in the scientist's usage. When astrophysicist's use the term "direct observation", they contrast this — i.e. they call unobservable — with information that they are aware of but aware of via other means, through a process of several inferences. For examples of the former — direct observation — Shapere points to the electromagnetic information that we can access from the surface layer of the sun; for an example of the latter, Shapere points to any region of the sun that is deeper than this surface layer. Here, for various features of the properties of stars and light, the electromagnetic radiation will likely never interfere with us in a way that we can be said to observe it how it was in that stage. Instead, our knowledge of these regions are indirect or inferential in nature.

Evans and Thébault (2020) discuss something very similar to this, but both maintain the language of the philosopher regarding observable and unobservable, and greatly expand on different types of unobservable. As can be seen from the image below (fig.1), unobservable can be split into either (a) manipulable, or unmanipulable. Further within unmanipulable, unobservable phenomena, is a division between (b) "accessible" and (c) "inaccessible" phenomena. Like Shapere, they maintain that a turn to scientific practice teaches us that a process of several different types of inference, which they call "inductive triangulation", are used within science to confirm theories. Again, what is observable to the senses plays no special role.

Massimi (2007) argues that the data models that van Fraassen formulates the observables in science around present not just observable but unobservable phenomena, also. They claim that we should instead understand phenomena in the sense that Bogan and Woodward (1988) use it. Data are directly observable and provide evidence for the existence of phenomena, and phenomena are detected through the use of data but in most cases are not observable (Massimi, 2007, p238-239). Massimi extends her view and argues that the evidence for unobservable entities that data provides comes from data that has been 'selected, regimented, and laboriously organized in a data model' (ibid, p240).

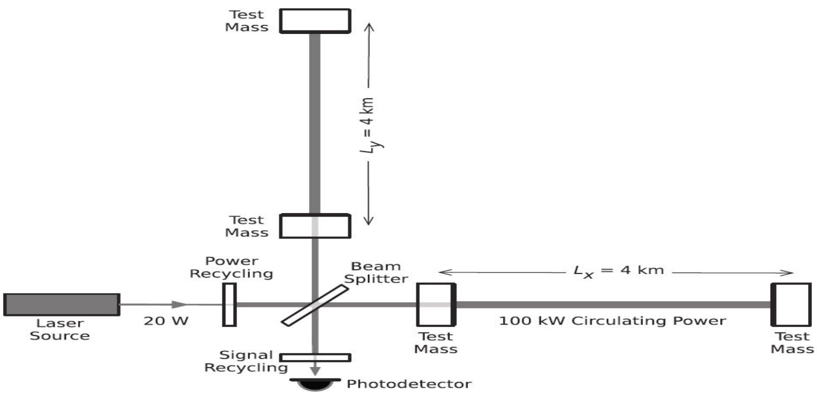
To support her case she turns to a case study of the discovery of the J/ψ particle in 1973/1974 by two groups of physicists led by Richter and Tang.[21] The particle was discovered, in the case of Richter's team, by the expected value of the relationship of hadronic jets to muonic jets being out by a scale factor of hundreds. In Tang's team, the particle was discovered by noticing that the electron-positron pairs had energies that peak around 3.1 GeV, with the peak being identified as a massive particle (ibid, p254). In Ting's case, many experimental checks were made to ensure that the particle cannot be attributed to noise or experimental error. Most importantly for the purposes at hand, the J/ψ particle is unobservable to our senses, but emerges to our understanding through a data model. Yet despite this unobservability to the human senses, scientist's subsequent aims were nonetheless to "save the phenomena" in the same way that they would for observable phenomena. Massimi details how a series of theoretical models were proposed, but either failed to save the phenomena (ibid, p258) or were forbidden by energy conservation (ibid, p259). The solution that the physics community opted for was the charmonium model, which was the one was most empirically accurate with respect to this particle. The point here is that *in practice*, the phenomena that the physics community will try and save is often unobservable to the human eyes.

[21]The unusual name is due to the two separate discoveries, with Richter naming it ψ and Tang naming it J.

Scientific evidence

A further problem with the privileging of sensory perception is this: if we are to epistemically privilege sensory perception, then we face a problem when it comes to how we should treat scientific evidence in that evidence that is unobservable to our eyes becomes of second importance, or even irrelevant, due to the fact that an epistemology of sorts presupposes what we do or don't count as evidence. Expanding this: before we decide whether or not a piece of data or a certain phenomena, x, supports a theory/model/experiment, we first need to decide whether or not x counts as evidence in the first place. If we take the view seriously that we should epistemically privilege naked-eye observation and draw the observable line at this level, then evidence that exists at the unobservable level is treated as second rate or perhaps even rejected.

While perhaps this may have been suitable for science of a bygone era, this cannot be suitable for a current science where we routinely have unobservable evidence. For just one example, we can turn to the recent (2015) detection of gravitational waves by two detectors of LIGO (Laser Interferometer Gravitational-wave Observatory). LIGO is comprised of two giant laser interferometers which are located thousands of kilometres apart. These intereforemeters each consist of two large arms (approximately 4 kilometres) which are perpendicular to one another, and a laser beam is shone through and reflected by mirrors at each end. The gravitational waves cause the arms of the interferometer to lengthen and shrink, with one getting longer while the other shrinks, and then vice-versa. When this happens, the time the laser beam takes to travel through the arms differs, which leads to the two beams becoming out of sync and an interference pattern being created. (See image below)



The "gravitational wave-version" is what physicists are measuring, and this is the difference between the two arm lengths in their movements, caused by the gravitational wave. This gravitational wave-version is far, far smaller than the eye can observe (1/10,000th smaller than the width of a proton); but they are measurable and detectable by these instruments just described. We analyse the data produced by these movements to determine that the experiment has been significant and that we have detected a graviational wave. Thus, both the phenomena (the gravitational wave) and the piece of evidence (the gravitational wave-version being significant) are unobservable. It seems clear that the latter should not be treated as second-rate evidence.

These points are sufficient to show that in practice science frequently relies on what is unobservable with the naked eye. In the previous subsection an ultimatum was given and the provisional claim was made that if one is willing to adopt this unobservable structure of counterfactuals, then one can draw the line wherever one wishes and remain internally coherent. The purpose of *this* subsection has been to show that despite this internal coherency, this is simply not a position that is compatible with modern science.

4.4.5 Scientific Instruments as Extension of Sensory Perception

So far, in this section, it has been established that the observable/unobservable line is not suitable to be drawn at the level of sensory perception, both from the perspective of internal coherency and also in practice. As mentioned at the beginning of the section, it may be tempting for one to shift this observability "line" back to what can be observed via instruments. The examples given there, under this idea, is that one can claim that we do genuinely perform an "observation" of some sorts when we detect neutrinos from the sun, or observe an atom when we "see" it in a cloud chamber, or that cells really are observable under a microscope. Van Fraassen denotes a helpful metaphor to this view, calling it the "window on the invisible word" (van Fraassen, 2008, p96) view.[22]

Van Fraassen says that this is a view that emerges in the Early Modern period, and cites Boyle's Micrographia and Power's *Experimental Philosophy* as being two instances within this historical period where this is made

[22] This is contrasted with his preferred view, the "instruments are engines of creation" view (van Fraassen, 2008, p96).

explicitly (ibid, p99). To this we can add the views of Francis Bacon and Robert Hooke.[23] More recently, Grover Maxwell expounds something similar to this when he argues that what were once thought of as theoretical (unobservable) entities must now in many cases thought to be directly observable, one reason of which is due to 'the use of instruments as aids to observation' (Maxwell, 1962, p14).

This goes some of the way towards satisfying the middle ground that we are trying to find between the aforementioned permissive and restrictive criteria — the **permissive** criterion being that we must allow for anything that could count as empirical evidence in science, and the **restrictive** criterion being that we want to remain empiricists in allowing only empirical evidence to count as scientific evidence. But solving the problem by extending what is observable to that which we can observe via scientific instruments is still *too restrictive*. It does not allow us to label as empirical phenomena and structures within science that seems just as undeniably to count. Phenomena such as spin, different flavours of quarks, the electromagnetic charge of a field, are all phenomena which we would want to place into this category and yet there surely will be no extension of our sight via scientific technology that could allow us to "observe" or "see" such entities. They are in this sense a different kind to things like atoms, electrons, cells, etc.

4.4.6 Measurement, not Observation

What should be given the greatest epistemic privilege is that which is measurable. By this I mean the class of phenomena that can be measured, and not those that actually have been measured. As will become clear, I advocate giving epistemic privilege to a specific part of the measurement process; the part that comes prior to the process of analysis. This will be made clearer shortly.

One may object that it is wrong to draw this sort of contrast between measurement and observation, since measurement is simply a special type of observation. This has certainly been the traditional view in the philosophy of measurement, whether this be in a foundationalist manner which seeks to

[23]Bacon frequently talks about experiments and scientific machines correcting our sensory perception and extending it. See Novum Organum, Book 1, aphorism L, for example. And Hooke writes in several places of this. See page 28 of this project for a direct quotation on this.

ground everything in measurements-as-observations (Campbell, 1920), or in other views such as (Bunge, 1998). But there are many measurements that do not rely on observation, and yet seemingly no instances of observations that could not be measured. For an instance of the former, EMF measurements are measurements on electromagnetic fields in the day-to-day (i.e. not under specific idealized conditions) that detect and measure electromagnetism via EMF meters. The fields themselves are not observable. The data from the reading is usually presented visually, and thus a very weak case may *prima facie* be made for some sort of observability claim, but there is no reason whatsoever that this must be the case; observability here is purely a pragmatic factor. The data could be presented in morse code, in binary code, in braille, or in whatever format that the user desire. Regarding the latter, any time we make observations, they can be measured in some important sense. The measurement of these observations would also *prima facie* only serve to increase their epistemic privilege: performing basic measurements like measuring the size of phenomena would rule out basic optical illusions that are created. Phenomena that are observable like hallucinations are surely not measurable, but this is only grist to my mill — we should not be placing epistemic privilege in such phenomena any way.

There are various approaches and areas of study within measurement. Tal (2020) lists (i) mathematical theories of measurement, (ii) the philosophy of operationalism and conventionalism, (iii) realist accounts of measurement, (iv) information-theoretic accounts of measurement, and (v) model-based accounts of measurement. Using the previously developed normative criterion of requiring that empiricism be practice-led — that is, focuses on what science and the scientific community is doing — we see that model-based accounts of measurement are the most suitable account to examine here.

Model-based accounts emphasise the relationships between measurement, and theoretical and statistical modelling. The accounts have been 'developed by studying measurement practices in the sciences, and particularly in metrology' (ibid). The motivation is cited as attempting to 'clarify the epistemological principles underlying aspects of measurement practice' (ibid). Due to the fact that this is the account of measurement that best accommodates scientific practice — a necessary criterion for an empiricism in philosophy of science, as per section (4.3) — this is the account that will be followed. In this account, measurement involves an interaction between the system being measured and the measurement system. Measurement represents these interactions with parameters, and assigns values to these parameters based on the interaction (ibid) ("quantities", when the results are numerical).

Before getting to the crux of my claim, it is worth very briefly examining some cases of model-based accounts of measurement. This will serve as an extensional definition of this approach, before giving a more intensional account of the approaches afterwards.

Approaches to Measurement

Parker (2017) presents three different types of measurements that are involved within an overall measurement process, which are heavily inspired by a combination of van Fraassen's (2008) and Tal's (2012) accounts of measurement. These are *direct*, *derived*, and *complex*. The difference between them lies in 'the layers of inference involved' when moving from the measurable states to the final measurement outcome (Parker, 2017, p279). She introduces the following terminology: an **instrument indication** is the state of an instrument/apparatus that is used in measurement; for instance, a pointer's position, or the digital display on a read-out, or the level of the liquid on a thermometer (ibid, p280). A **raw instrument reading** is the reading of this instrument indication (ibid). A **measurement outcome** is a necessarily selective representation of the measured system which is — importantly — inferred from one or several of the instrument indications.

A *direct measurement* is where the raw instrument reading immediately assigns a value to the parameter in question. There may be some calculation here when inferring a measurement outcome from the raw instrument reading, but these calculations would be made for corrections to account for extraneous factors, and do not fundamentally modify the reading (ibid). A *derived measurement* involves at least one extra inferential layer (ibid, p281). Here, measurement outcomes are calculated or derived from values that have been directly measured. Derived measurement can be either synchronic or diachronic. *Complex measurement* involves making several direct and/or derived measurements and combining these results to get a more detailed and informative measurement outcome than would be possible using the former two styles of measurement (ibid, p283). There are various ways that complex measurements are made in practice. Parker lists three: (i) combining multiple measurements of the same parameter, (ii) using multiple measurements that are used as a sample population and projected via some sort of statistical inference on to the target population, (iii) taking various measurements, adding some additional structure to create a data model (ibid).

Morrison (2009), to give an example of her account of measurement, pointing to an example of the physical plane pendulum that can be used to measure local gravitational acceleration. We make the measurements using this device, but a large number of corrections need to be made in order for this measurement to be accurate (2009, p49)[24]. Some of these corrections involve complex applications of models, and thus there is extensive use of theoretical models within this measurement such that it would not be able to be said to perform a measurement without these models — 'the ability of the physical pendulum to function as a measuring instrument is completely dependent on the presence of these models' (ibid, p35). It is then in this sense, she argues, that models function as measuring instruments (ibid). Summarised in her words: '[w]ithout models, there is no measurement' (ibid, p50).

Tal (2017) presents an epistemology of measurement, arguing for a model-based approach to measurement as a methodology and to any epistemology of measurement. He begins with a division between two sorts of measurement outputs: (1) *instrument indications/readings* (2017, p235)(2020)and (2) *measurement outcomes/results*(2017, p235)(2020). The former are the raw data, so to speak. It is what the measuring instrument displays, or 'the measuring instrument in its final state after the measurement process is complete' (2017, p235). The latter are epistemic claims about the values of the quantities attributed to the object being measured. They are the processed data, and involve background knowledge, inferences, and various other theoretical and statistical assumptions (ibid). The first is the 'level of concrete interaction' (2017, p240), and the second is 'the level of abstract representation' (ibid). Moving from (1) to (2) is a necessary component for producing both a final measurement outcome, and in securing any epistemic claims that we want to make about the system being measured.

This division is of vital importance for the claims being made here, and can be seen also in Morrison's and Parker's account. For Morrison, it is framed in terms of the initial measurement with the pendulum, and the corrections that are made to the measurements via complex applications of models. For Parker, her notion of direct measurement corresponds to (1) in Tal's categorisation, and her notions of "derived" and "complex" measurements correspond to stage (2) in Tal's categorisation.

What should be given greatest epistemic privilege is (1), and not (2). Given that Tal stresses that '[i]nstrument indications do not by themselves provide any objective knowledge, that is, knowledge about the objects intended to

[24]These include making (i) finite amplitude corrections, (ii) mass distribution corrections, (iii) effects of air including buoyancy, damping, added mass, and theoretical damping constants, and (iv) elastic corrections due to wire stretching (Morrison, 2009, p49).

be measured' (Tal, 2017, p240), this may strike one as odd. To be clear, it is *not* my intention at all to claim that the initial stages of measurement are where we will learn most about the measurement, or about the phenomena. My point, rather, concerns the class of phenomena that are measurable. *If something can be measured via this concrete interaction with a measuring device, then we should epistemically privilege it.*

Much theoretical work is done to move from (1) to (2). An iconic and extremely simple example is Bogen and Woodward's (1988) use of Nagel's (1979) example of measuring the melting point of lead by taking a reading from the thermometer; to do this is not a simple, single instance, but requires many readings, where the readings are plotted onto a scatter graph and the mean is taken. An inference is then made that this point — which could very well not have actually occurred in any of the instances — is the melting point. The precise details of how we move from (1) to (2) is not of vital importance for the point I'm making here; all that is important is that there are many assumptions that go in, and various procedures that in practice require the use of models to obtain a result.

In Bogen and Woodward's borrowed example, what I claim should be given greatest epistemic privilege is the phenomena of melting lead. In Morrison's example, it is the phenomena of local gravitational acceleration. The process of moving from this singular instance of taking a measurement to the various processes that occur between (1) and (2) evidently lose some epistemic security in that we are making various assumptions which we are sure create some sort of uncertainty in our final outcome. This is *not* to say that we shouldn't be secure in the result of our final measurement outcome; it is just to say that we should not treat it as being as epistemically secure as should be our faith in believing that lead has a melting point.[25]

To be clear: there certainly should be a good amount of epistemic weight that we should assign to (2). *But*, and of high importance, *any epistemic privilege that we should give to (2) is purely in virtue of ensuring that (1) is done adequately.* (1) is foundational to (2) in this sense. The first part of this measurement must be accurate as a necessary presupposition for anything else to be able to have any sort of status as epistemically privileged. (1) Will typically involve multiple interactions between measuring device and phenomena, and despite there being procedures which can strip away anomalous results,

[25]This was stressed in the introduction to this section. When I speak of epistemic privilege, I am speaking in terms of degrees and not of a binary scale of privileged vs not privileged.

the final measurement outcome is nonetheless foundationally dependent on the initial measurements. The second part of the measurement process — the analysis and various statistical procedures that we make, and the final result that we get — should certainly be epistemically privileged also, *but only if (1) is done adequately.* For them to hold any reason for epistemically privileging them it is only in the case that the initial part of the measurement is accurate. If we are trying to measure the charge of an electron and fail to make the initial measurement properly, then all the sophisticated processing and analysis in the world should not lead to us epistemically privileging this.

Privileging measurement is different to privileging observability. The claim that lead does have a melting point, and epistemically privileging facts such as this, cannot be accounted for when giving greatest epistemic privilege to the observable — whether that be observable with sensory perception or observable with instruments. We can examine another example in science to illustrate this claim in clearer terms, and can turn to the example of the detection of gravitional waves in the previous sub-section. Although physicists here are conducting the experiment with the aim of detecting gravitational waves, the phenomena that they measure is the "gravitational wave-version", which is the difference in the movement between the two interferometer arms. This is supposedly caused by the gravitational wave, which is inferred via an inference to the best explanation (IBE). Here, the phenomena of the "gravitational wave-version" is what should be epistemically privileged, under this proposed framework. And to emphasise — this is not to say that we shouldn't place epistemic security in gravitational waves, or withold belief in them; simply that they are not as epistemically secure as the phenomena of gravitational wave-versions, and thus that they should not be epistemically privileged *to the same degree as the phenomena that is being initially measured.*

The benefit of replacing epistemically privileging observables with this specific stage of measurement is that it provides a far better solution to balancing the two desiderata of **restrictiveness** — where we want to restrict what we epistemically privilege to the empirical realm — and **permissiveness** — where we want to be liberal enough to allow for all phenomena and processes in the ever-expanding domain of science.

Introducing measurement also has the additional benefit that it is a collective process, as opposed to individualistic. This satisfies criterion (1) of this chapter. Privileging sensory perception is thoroughly founded on individualism. The motivations for basing epistemic privilege around sensory perception were highlighted above, and there is a distinctively individualistic characteristic to them. Further, the very idea of it is born and cemented in the Early Modern time with Descartes' individualistic conception of an individual looking out into the world to try and determine what they can know. The Early Modern empiricists all talk in this style, as has been documented in chapter one. Measurement avoids this, and is an inherently collective process whereby the community is involved in the various stages, including the stage that I propose privileging. The process of measuring requires a whole team of scientists to perform and to verify, and cannot be said to be inherently tied to humans.

Is This Circular?

If what is measurable is what should be epistemically privileged, then there seems to be some degree of circularity here. This can be sketched out as follows, and requires two background assumptions:

(BA1): Measurement is a success term in science. To measure something is to epistemically privilege it.

(BA2): [Based on section 4.2]: We should turn to scientific practice to derive the notions and concepts that science uses, and not let philosophy dictate these terms to science.

Then, we have the claim made earlier:

(C): Measurement should be epistemically privileged.

A question naturally arises from this — what counts as a measurement? To answer this we should turn to science, and thus answer something like "whatever science says counts as measurement". Importantly, because of (BA1), and measurement being a success term, the answer is equivalent to as: "whatever science epistemically privileges". So the argument can be rephrased in more abstract form to highlight the circularity:

(1) Measurement should be epistemically privileged in science.

(2) To find out what a measurement is, we should turn to science.

(3) Science says that a measurement is whatever science epistemically privileges.

(c) Therefore, a measurement is whatever science§ privileges.

We should not, though, see this as a vicious circularity. This is avoided by understanding what counts as measurement as being temporally-dependent. What counts as measurement is whatever science says counts as measurement *at a specific time*. The standards of what does or does not count as a measurement have changed over time in science, and we would be wise to assume that they will change once again. Thus, what science 100 years ago epistemically privileged qua measurement is different to what science now epistemically privileges qua measurement, and therefore this apparent truism that is at the heart of the circularity is not vicious.

There is an objection that should be addressed: surely sensory perception of medium sized objects, providing we can cross-reference these with a large enough group of people, provides more epistemic security and thus should be epistemically privileged more than the measurement of various phenomena in science which cannot be observed with sensory perception. But this class can comfortably be accomodated into the category of measurement — anything that is observable via sensory perception can be measured; and, indeed, if one is to proceed to measure this then this adds epistemic security to this object. We also have robust methodologies for eliminating artefacts from the measurement process, of which we do not have within perception of medium sized objects; measurements are thus not subject to illusions in the same way that day-to-day perception is.

4.5 Causal Realism

(4) Empiricism should embrace some form of realism about causality.

This section argues that an account of causation is required for any empiricism that is suitable for science. On the face of it, this is far better accounted for via a realist framework than it is via anti-realist accounts, and thus this section argues that causal realism is the most suitable option here. The realist explanation is simply that the world, and science, appears to be full of causes simply because causality is real, anti-realists have a far harder job to account for this. The burden of proof should be on the anti-realists, and not on the realist, to account for the phenomena of causality that permeates science and the world more generally. With that being said, the object of criticism in this section is not of anti-realist accounts *en masse*, but, rather, anti-realist accounts that seek to dismiss causality as a folk science.

This criterion can be derived from a combination of (α) **empirical investigation**, (β) **de-centralising of sensory perception**, (δ) **practice-led**, and (γ) a **willingness to endorse metaphysics**. When we **empirically investigate the world** we see that there is evidently some notion of causality that is needed to understand it. By performing this empirical investigation we realise this from being able to create more abstract notions (cause and effect) from what is directly perceived via sensory perception, and thus **decentralise sensory perception**. If we focus on what happens in practice (**practice-led**), we clearly see that an account of causality is needed to be able to explain the world, and if we focus on scientific practice then scientists are constantly operating with a framework that presupposes causality. Finally, by allowing a **willingness to endorse metaphysics** we can comfortably embrace this notion of causality. The arguments in this section also pay great attention to criteria (2) and (3) presented here — the criterion of the importance of focusing on practice, and the criterion of epistemically privileging measurement over sensory perception. The arguments made to an extent presuppose these two criteria, and argues that given this, we need causality.

Before all this, though, it should be said what is meant by causality. It is not within the aims or scopes of this section to try to give a coherent account of what causality is, or what the nature of a cause is. There have been various different attempts to do this; Woodward's (2016) overview of causation lists regularity accounts proposed by those such as Mackie (1974), counterfactual accounts such as those proposed by Lewis (1973), causal process theories such as those proposed by Salmon (1984) and Dowe (2000). The characterization given by Frisch (2009a, p460) is sufficient for the purposes at hand here:

(i) Causes determine their effects.

(ii) Causes act locally.

(iii) The notion of causation is asymmetric.

Some philosophers, such as Russell (1914) and Norton (2003), characterise causality as requiring a necessary connection between cause and effect. But this need not be the case, and many causal realists have embraced a probabilistic form of realism whereby the necessary connection between cause and effect is replaced by a probabilistic account — if the cause is present, the probability of the effect being present is increased (Suppes, 1970)(Salmon, 1984) (Skyrms, 1980).

Causation as Folk-Science

There is a variety of anti-realism about causality in philosophy of science that originates in Russell's 1914 "On the Notion of Cause". Russell argues here that causality doesn't exist. Both causes themselves, and any sort of "law of causality". But not only that, he argues that the concepts of causality, and the language that surrounds it, is fundamentally problematic and 'bound up with misleading associations' (1914, p1); so much so that we should entirely remove it from our philosophical vocabulary. The reason for this is entirely naturalistic in its motivation — Russell turns to what he thinks is the most advanced science of his day (gravitational astronomy), and points out that the causal language doesn't occur in this theory. He famously declares that '[t]he law of causality... is a relic of a bygone age, surviving, like the monarchy, only because it is erroneously supposed to do no harm' (ibid). There is thus a descriptive claim, an ontological claim, and a prescriptive claim.

Descriptive: in our most advanced science, causality is not a concept that is used.

Ontological: There does not exist anything like causes, or causality, or causal laws.

Prescriptive: We should eradicate talk of causes from our philosophical language.

Regarding the descriptive claim, whether or not this was true in Russell's time, it is absolutely not the case now. Suppes', in his *Probabilistic Theory of Causality* (1970), writes:

'Contrary to the days when Russell wrote this essay, the words "causality" and "cause" are commonly and widely used by physicists in their most recent work. There is scarcely an issue of *Physical Review* that does not contain at least one article using either "cause" or "causality" in its title.'

(Suppes, 1970, p5-6).

Hitchcock takes up Suppes' point in more current times (2007), and finds 76 articles in *Physical Review* between 2000 to 2003 with the terms "cause", "causes", "causality", or something to this effect (Hitchcock, 2007, p55). To therefore claim that our most advanced sciences do not use these concepts is simply false. This has implications for his prescriptive claim, also. If scientists in the most advanced sciences are discussing causes and causality in a significant way, then there is no reason to banish this terminology from our philosophical language.

So: in scientific practice, causality is a concept that is evidently employed. Before turning to the ontological claims that Russell presents, another important characteristic of ME is relevant to explore. This is that of **empirical investigation**, and is the driving force of ME. It is the way in which experience is primarily used in ME — that we should empirically investigate the world. If we are to empirically investigate the world, we find that causality is similarly crucial; this cannot be done without some notion of causality. We can turn to Radder and Hacking to demonstrate this point.

Radder (2021) argues that scientific practice presupposes causality.[26] The general idea of the main argument is very simple: when we perform scientific experiments, and in general scientific practice, we literally cannot do this without presupossing some form of causality. Specifically, the following three points must be true in order to gain sound empirical knowledge from science:

(i) We must assume that there likely exist external factors that can causally influence the experimental systems at hand (ibid, p602)

[26]The paper also presents a crude version of empiricism that is limited to EE, and argues that this fact — that science requires causality — damns empiricism generally. This part of the paper will not be of interest here.

(ii) We must succeed in knowing which are said relevant causal interactions (ibid).

(iii) We must be able to prevent or eliminate these causal interactions by actual interventions (ibid).

Radder gives three examples which highlight this. The first is an immunology lab where experiments are performed that do not match the results of other labs that are performing the same experiments. It is discovered that there is an external factor that is causing the experiments to do this; the external factor is removed and the experiments align with the results of the other lab (ibid, p601). The second is in astronomy — 50 large windmills were planned to be built around seven kilometres away from the largest radio-telescope in the world (ibid). Astronomers objected to the building of these on the grounds that the windmills would causally affect their experiments as the rotating blades would disturb the radio signals that they were measuring. The third is the aforementioned experiment of the measurement of gravitional waves, where the effect was measured, and this would only make sense as scientific knowledge if the effect was caused by gravitational waves.

Hacking (1983)(2013) presents a similar case with his entity realism. He presents something akin, in structure, to a transcendental argument, which examines a "given" and investigates what the necessary preconditions are for this given to exist. For Kant, the transcendental argument was framed around experience, for Hacking, the given is scientific instruments. Given that this scientific instruments do exist and work, it is necessary that certain entities (electrons, atoms) do exist because the scientific instruments depend on these entities existing for the scientific instruments to work (2013, p757). Scientific instruments are designed depending on the causal properties of these various unobservable entities.

We should also bear in mind here criterion (3) — the epistemic privileging of measurement. Taking this seriously leads to further confirmation that one must hold to some form of causality. Calibrating instruments, differentiating artefacts from phenomena, and other such processes in scientific practice in how we do measurement would be impossible without positing causality. The readings that we get from measurement devices that we infer to be reliable information are presumed to be caused by the world, or by the phenomena that we are measuring; if it were not, then we would surely have no reason to trust it.

Causal Realism or anti-realism?

The above is sufficient to establish that when we turn to the world itself via empirical investigation, causes are found and required. In response to Russell's ontological claim, and the more metaphysical question of the existence of causes and causality, there are two different ways that we can proceed. The *first* is to treat causality as a necessary framework to operate with and to do science, but one that is not actually real. Under this approach, it has great pragmatic value but does not actual have metaphysical reality. The *second* is to treat causality as real.

The first forms a prominent anti-realist position within the philosophy of science, and has lead to volumes such as (Price and Corry, 2007) that argue for what is termed "the republican option". Specifically alluding to Russell's reference to the monarchy, they argue that dispensing with causality altogether via eradicating it from our vocabulary is akin to jumping from the monarchy to anarchism. They instead propose a middle ground:

'Causal republicanism is thus the view that although the notion of causation is useful, perhaps indispensable, in our dealings with the world, it is a category provided neither by God nor by physics, but rather constructed by us'.

(Price and Corry, 2007, p2)

Various different positions are presented that defend, in some form or other, this framework. One such is Norton's (2003), who argues that causality is a highly useful concept, but denies its existence. He thus differs from Russell in the claim regarding causality's pragmatic value. Norton is, in a similar vein to Russell, motivated 'by taking the content of our mature scientific theories seriously' (2003, p2). He argues specifically against a principle which he calls "causal fundamentalism" (2003, p3). This is the view that:

'Nature is governed by cause and effect; and the burden of individual sciences is to find the particular expressions of the general notion in the realm of their specialized subject matter.'

(ibid).

He argues against this principle by presenting a dilemma. *Either* conforming a science to causality places a restriction on the factual content of a science, *or* it does not. If it *does* place a restriction, then we must be able to find such a restriction on factual content that can be applied to all sciences. In other words, there must be a causal principle that rules out certain empirical facts occurring within science. If it does *not* place a restriction, then Norton argues that it is simply an 'empty honorific' (ibid, p4), since no work is being done by causality.

Norton sees the first horn as being demonstrably false, and that this is demonstrated by physics. If the world is causal, then it must be ruled a priori both action at and non-local processes. But, he points out, both do occur in physics; the former in the standard view of gravitation in science in the 19th century, and the latter in Bell's theorem in quantum mechanics (ibid, p6). There are also examples of acausality in classical physics, like Norton's dome — first introduced in this paper — which displays indeterminism in classical physics. The second horn, as has been stated, he sees as doing no philosophical heavy-lifting, and thus honorific.

But Norton's account is too heavily focused on theories, with no proper reflection on scientific practice. This directly clashes with the practice-led characteristic of ME and normative criterion presented in (4.3) of this chapter. Arguments that rely on theory in isolation from practice are not relevant to ME. There are arguments that address Norton's claims explicitly and on their own terms, though. That is to say, appeal also to theories taken in isolation to rebut Norton. For instance, Frisch (2009a) responds directly to Norton's anti-realism by examining dispersion relations that are used in a variety of theoretical physics to characterize scattering phenomena (Frisch, 2009a, p462). [27]

These anti-realist accounts that insist on the pragmatic utility of causality are motivated by a desire to not introduce surplus metaphysics. ME need not be motivated by such a desire, provided the metaphysics are naturalistic, which here they clearly are. As has been shown, a core characteristic of ME of a general **willingness to allow metaphysics**. This attitude that is presented by this "causal republicanism" framework is thus against the spirit of ME. ME would not rule out this option altogether, purely on this basis; but it is far easier to embrace a realism about causality here than it is not to.

[27] He points out that these are characterized as 'causality conditions' (ibid) in the physics literature, and focuses specifically on dispersion of light in classical electromagnetism as a case study to argue that certain parts of the theoretical framework are either directly implied by or express a causality condition which can be used in various areas of physics (ibid, p462-464). Frisch doesn't focus on scientific practice, but examines different sets of equations within classical electromagnetism to search for this causal principle, and discusses physicist's analysis of said equations.

To introduce an anti-realist account here requires much philosophical work in order to offer explanation of why the world appears to be causal and yet, in fact, causality does not exist.

There is also a normative component of this chapter. As well as finding the most suitable empiricism for ME in current science, derived from these characteristics of ME, it is claimed that this is the proper empiricism to embrace, given current science. For this, two points can be turned to. The first is an appeal to both a sort of Ockham's razor and an inference to the best explanation: surely the *simplest* explanation of causality appearing to exist is that causality does exist. And surely the *best* explanation for causality appearing to exist ubiquitiously across the sciences in scientific practice is that causality really does exist. The second is that realism about causality does great explanatory work that holding an anti-realist position does not do. These points, taken together, are enough to place the burden of proof squarely on the anti-realist to give a proper account of *why* there appears to be causality, as opposed to having the burden of proof on the realist to explain why they think causality is real. Given the evidence that has been presented, the default position should be one of causal realism. An analogous, everyday scenario, illustrates this point well. I hear a smash from the kitchen, and when going in to investigate I see broken glass spread across the kitchen floor, shattered into pieces; there is water on the floor that is quickly spreading. My friend enters the room at the same time. I argue that the broken glass and wet floor is caused by the glass of water that I left slightly precariously balanced on the kitchen top five minutes ago and forgot about; they argue that it wasn't, but do not dispute the fact that this appears to be the case. They could be correct — but evidently the burden of proof is on them to account for this phenomena, and not on myself to argue my case. The reason why the burden of proof is on them is because my account is far, far more straightforward and explanatory, as per the case highlighted just now of why advocate for causal realism.

Conclusion

This chapter continued the focus on ME, this time shedding light on what ME would look like in the context of current science by taking four normative criteria from the characteristics that were presented in (1.1). It was also argued that each of these criteria are desirable in and of themselves, and that they form the most suitable empiricism for current science. There is thus both an explorative dimension and a more purely prescriptive dimension to this chapter. The explorative dimension is seeing where we arrive if we apply ME to current science, and exploring the consequences of this. The prescriptive dimension argues that this is the best way to formulate empiricism.

In order of appearance, the four criteria were that (i) empiricism should prioritise the collective over the individual; that (ii) empiricism should prioritise practice over theory; that (iii) empiricism should epistemically privilege measurement, and not observation; and (iv) that empiricism should embrace some sort of realism about causality. Many arguments were presented for them.

Importantly, whilst I claim that this is the best way to apply ME to current science, and that this is the position that empiricists should endorse, this version of empiricism that has been presented here is not a reiteration of any other empiricism that has so far been developed. It has many similarities to other positions in ME, particularly that of Cartwright's. But even with respect to this there are significant differences. She does not discuss the individual or the collective explicitly, whereas I claim that it is an important point that must be discussed within empiricism. She rejects the view that measurement or measurements are of foundational or supreme status within a philosophy of science (personal correspondence)(2021), preferring instead to give "empirical facts" this position. I claim that measurements occupy this place. Further, she does not want to introduce any sort of epistemic position into her empiricism. As can be seen from the discussions of measurement, epistemic notions — specifically epistemic privilege — play an important role in the empiricism that I advocate.

Chapter 5

"Non-empirical" Approaches in Philosophy of Science: A Threat To Methodological Empiricism?

Introduction

There has been much quality work done in the philosophy of science in the last ten or so years on various parts of science that seem at least *prima facie* to be non-empirical in some sense. In the title, there are quotation marks placed around "non-empirical". This is because, as will be seen through this chapter, I do not believe that all of these are truly non-empirical. The studies have primarily been in the form of (i) analogue confirmation in science, (ii) computer simulations, and (iii) more traditional examination of direct non-empirical theory confirmation, predominantly by Dawid. *Prima facie*, the conclusions drawn from these topics — that we can have some sort of non-empirical confirmation of theories or non-empirical method in science — seem problematic for an empiricist position as outlined in chapter 4. If science is relying in significant ways on *any* sort of confirmation that is non-empirical, or on non-empirical methods, then this seems to directly go against empiricism. This chapter aims to address this apparent threat.

One may be confused as to why I have set out for a need to focus on practice, and yet am addressing theory confirmation as a key element in this chapter. The reason for this is that I am directly examining a literature that seems to pose a threat to empiricism, and this literature focuses heavily on such confirmation of theories. It is negative, not positive, philosophy that is being done here.

The "non-empirical" part of this subject should be clarified before properly beginning; and in particular it should be made clear that it is not as threatening as it first appears. In the work of analogue experiments, it refers to empirical evidence but empirical evidence that lies outside the empirical domain of the particular theory. In Dawid's (2013)(2015)(2019) work on non-empirical theory confirmation, there are various more genuinely non-empirical factors, but argues that this kind of evidence can only ever confirm to a certain extent, and can never provide conclusive evidence to warrant one accepting a theory. One still needs empirical evidence for the theory as essential. This is, of course, a cursory overview, and this will all be much expanded on in the main body of this chapter.

5.1 Analogue Confirmation

The recent work done on analogue confirmation in philosophy of science originates in Dardashti, Thébault, and Winsberg (2017), although this builds on work done by physicists such as Unruh (1995)(2008)(2014). In their (2017) they set out a framework by which analogue experiments performed in physics can be said to be confirmatory. The form is the following. The less accessible system that we seek to learn about but cannot empirically access is denoted the target system, T. The more accessible system that we can access and draw conclusions from is denoted the source, S (Dardashti et al, 2017, p65). D denotes the domain of conditions that the model is applicable to (ibid, p66), and M denotes the modelling framework that is used to build a particular model. Their set of criteria that must be fulfilled in order to qualify as an analogue simulation is as follows:

Modelling adequacy: The modelling frameworks for both the source and the target system, M_S and M_T, are adequate within a certain domain of conditions, D_S and D_T.

Sufficient Mathematical Similarity: There exists enough mathematical similarity between the structure of M_S and M_T so that a syntactic isomorphism can be defined within domains D_S and D_T.

Inability to Access Target System: We are interested but unable to access a specific fact or property of T within D_T.

Ability to Access Source: We are able to study S.

Which leads to the final two claims that (i) a system S will exhibit phenomena P_S, within D_S and to a specific degree of accuracy; (ii) analogously, a system T will exhibit phenomena P_T, within D_T, and to a specific degree of accuracy. In less formal terms: 'in certain circumstances, analogue simulation can provide inductive support for a hypothesis regarding the target system, on the basis of empirical evidence regarding the source system. In other words, it can give us confirmation.' (ibid, p68). They are not concerned here with any particular type of confirmation theory, but others have taken up this issue with, for example, Bayesian accounts of confirmation in analogue simulation (Dardashti et al, 2019).

We also need, they claim, reasons external to the modelling frameworks in question and empirically-grounded arguments to be able to justify the similarity between S and T (ibid, p70-74). They denote this "MEEGA" (model-external and empirically-grounded arguments). In Dardashti, Hartmann, Thébault, and Winsberg (2019) (henceforth (Dardashti et al, 2019)), this concept of MEEGA is expanded on via a more explicit rendering of universality arguments. Universality is defined as the 'insensitivity of a phenomenon under a type-level variation between systems with fundamentally different material constitution' (ibid, p4). Thus, universality arguments between the source and target system are necessarily presupposed for any analogue confirmation, making universality arguments of utmost importance for analogue confirmation. They also make clear why we should believe the Hawking effect to be justified via universality arguments, and the process of analogue confirmation is also given a formal rendering via Bayesian confirmation theory.

Crowther, Linnemann and Wütrich (2019) argue that the framework proposed in Dardashti et al (2017)(2019) is circular in that analogue experiments must presuppose the adequacy of the modelling framework that is used to describe the target system. Evans and Thébault (2020) provides, amongst other things, a detailed response to Crowther et al (2019). In it, a more general account is given of the boundaries of what we can learn from experiments, where analogue experiments consist of just one type of experiment that they are concerned with. The concern is on the boundaries of experimental knowledge, with the argument being that the domain of what an experiment can confirm is *not* set by the conditions of what can observed (in the philosophical sense of perception), nor manipulated, nor what can be accessed directly via the experiment, as is perhaps traditionally thought of in experiments. Instead, they claim that the limits of what knowledge experiments can give us about the target systems are set by how far "inductive triangulation" can take us. Indeed, part of what makes an experiment externally valid (i.e. it justifies our beliefs about a target system that we can learn from an experiment (Evans and Thébault, 2020, p2)) typically includes as a condition sucessful "inductive triangulation" in order to mitigate reasonable doubt that one may have about the inference made from source to target system (ibid, p8).

Explicitly in response to Crowther et al (2019), they claim that whilst the clasically-formulated problem of induction relies on just one principle of uniformity for its charge of the circularity of induction, Evans and Thébault turn to work from other philosophers to demonstrate that there are in fact a plethora of them.[1] Temporal uniformity, spatial uniformity, intra-type uniformity, etc. This diversity thus serves to turn the circularity problem into an justificatory regress problem, since each uniformity principle must be justified. The use of several different inferences that use different uniformity principles they call 'inductive triangulation' (ibid, p5), and claim that these types of inductive inferences can and do serve in scientific practice to gain knowledge of phenomena. Using this framework, they claim that Crowther et al's argument amounts to no more than inductive scepticism.

Gryb et al (2021) examines universality arguments more generally, arguing that universality arguments for Hawking radiation are not successful; especially so when compared to the universality arguments used around Wilsonian renormalization groups within condensed matter physics (ibid, p809).

Field (forthcoming) attempts to resolve the tension at play here, and aims to present criteria for what constitutes a successful universality argument. Due to the foundational nature of universality arguments to analogue confirmation, this is a crucial issue to the latter. She distinguishes between two notions of success for universality arguments — "strength" vs "relevance" (ibid, p1). An argument is strong if it is probable that its conclusion follows from its premises (ibid, p20); naturally, the stronger an argument the more probable it is that the conclusion follows from the premises. A universality

[1] Sober (1988), Okasha (2001)(2005), Norton (2003).

argument is relevant if it is 'known to be significantly positively relevant to the systems of interest' (ibid). Her main conclusion here is that we are able to construct successful universality arguments if: (1) 'we know enough about how that domain's micro-physics is structured' (ibid, p1), or (2) 'we are able to empirically test the domain's macro-behaviour' (ibid).

5.2 Computer Simulations

Winsberg (2019) points out that there are two typically used definitions of computer simulations in the literature — the "narrow" definition and the "broader" definition. Winsberg (ibid) defines them in the following way: the *narrow definition* defines a computer simulation as being a programme that is run on a computer that uses methods to explore the behaviour of a mathematical model, which is typically a model of a real-world system. The *broader definition* conceptualises computer simulations as a comprehensive method for studying systems. When we talk of computer simulations, we are talking about the whole process of choosing a model, finding a way to implement that model so that it can be run on a computer, calculating the output of the algorithm, and visualizing and studying the resultant data. It is typically this understanding of computer simulations, he says, that philosophers have in mind when writing and discussing computer simulations in the philosophy of science.

There are two types of computer simulations that are typically studied — equation based simulations and agent-based simulations (Winsberg, 2019). The former is most commonly used in the physical sciences and in sciences where there is reliable, well-formed and well-trusted mathematically formulated theories that guide the building of models based on differential equations. The latter are common in the social and behavioural sciences, and have no differential equations that govern the simulation. Both are used for (i) predictions, (ii) understanding, and (iii) heuristic purposes. Agent-based simulations are discussed far less in the philosophical literature, and consequently it is on equation-based simulation that this chapter will focus on.

Of the former — equation-based simulations, we can give the following sketch, based on Winsberg (1999)(2001). These equation-based simulations usually focus on complex, non-linear systems, and aim to give scientists a greater understanding and ability to represent the phenomena studied (2001, pS443). Winsberg gives examples of a storm, or a gas jet as two among many. The systems in question have an underlying well-established theory via physics that is well-tested and well-trusted. From this theory we will get a set of partial differential equations that govern the dynamics of it. Where computer simulations are used will be for systems where said equations are unsolvable by analytic methods. So the computer simulationists turns the differential equations into *difference equations* — equations that are finite/discrete rather than infinite/continuous — in order to be able to theoretically solve them by brute force. But — and this is crucial for all the philosophical significance of computer simulations — in practice this is not possible to do without using models that introduce various not insignificant modifications.[2] Simulationists have to make various ad hoc assumptions which are all philosophically interesting, including but not limited to (i) simplifying various assumptions, (ii) removal of degrees of freedom, (iii) substituting various empirical relationships for more complex but more theoretically founded laws (Winsberg, 2001, S445).

Two important concepts that concern the computer simulations accuracy are **verification** and **validation**. Verification is concerned internally with the model's accuracy *with respect to the original mathematics.* Validation is concerned external with the model's accuracy *with respect to the target system.* Both procedures are used in the process of computer simulations in order to ensure the accuracy of the simulations with respect to both of these factors.

As can be seen here, but is further emphasised and expanded in Winsberg (2009), equation-based simulations aren't typically concerned with theory confirmation/disconfirmation. A lot of theory is presupposed to build the model that the computer simulation runs, and so the simulation would not be suitable to test it since it presupposes it. They are concerned typically with understanding and representing of phenomena and of theories.

Winsberg (2019) draws out the *identity* (ID) thesis and the *epistemic dependency* (ED) thesis in the literature on simulations. These are two related but importantly different positions. The ID thesis is a statement regarding the identity of both experiments and simulations, and claims that computer simulations literally are a subset of experiments. The ED thesis is an epistemic statement about the significance of the ID thesis, if it were true. It claims that the degree to which a simulation can justify a belief in a hypothesis is

[2] The inability to in practice do this is due to the insufficient time and computer power that we have available to us with our current technology.

in some way directly proportional to the similarity between simulations and experiment. Three types of this are identified, again, in Winsberg (2019):

Weak ED: the ID thesis, if true, would be a *good* reason to believe that simulations can provide warrant for belief in H.

Stronger ED: The ID thesis, if true, would be the *best* reason to believe that simulations can provide warrant for belief in H.

Strongest ED: The ID thesis, if true, would be the *only* reason to believe that simulations can provide warrant for belief in H.

The "if true" qualification is important to stress, and changes the nature of the relationship between the two. The ED thesis presupposes the ID thesis in some important sense, for if one is to assert the ED thesis in any form it is clearly at least referring to the ID thesis. However, because of the hypothetical nature of the ED thesis — marked out by the "if true" qualification — it is not a necessary precondition of holding the ED thesis that one must also hold the ID thesis as well. For example, one can reject the ID thesis but hold to the ED thesis given that the ED thesis is hypothetical, but this relationship would not be possible if the ED thesis required a presupposition that the ID thesis is real.

As Winsberg (2019) points out, an early advocate of a combination of the ID thesis and the ED thesis is Norton and Suppe (2001). They argue that simulations can warrant belief *because* they are experiments. A valid simulation, they claim, is one in which certain formal relations hold between a base model, the modelled physical system, and the computer running the algorithm (Winsberg, 2019)(Norton and Suppe, 2001, p73).

Parker (2009) defends the ID thesis. She argues that computer simulation *studies* do qualify as some sort of experiment.[3] Whilst the two do have differences — a simulation is a type of representation, whereas an experiment crucially involves intervention — they do not have mutually exclusive extension terms (2009, p487); in other words, there are various phenomena that can qualify as both. Parker argues that what is important in the epistemic significance of experiments is also not their materiality, but similarity between the experiment and the target system. Whilst their is undoubtedly

[3]This is different to computer simulations themselves, and is far more broad. It involves the whole process of creating the simulation, running it, analysing it, etc. It broadly corresponds to what Winsberg (2019) refers to as the "broader" definition of computer simulations.

a positive correlation between the two — materiality does tend to produce epistemic trustworthiness — this is not the end but the means to obtain the ends, and the right type of computer simulation studies can facilitate good similarities between the source and target.

Parke (2014) argues for something similar: there are good reasons to think that experiments do have a greater epistemic privilege than computer simulations, or even simulations in general. But this often becomes blurred with a more general methodological question that people tend to create a black and white situation out of — that experiments are simply always better for generating scientific knowledge (Parke, 2014, p518). She wants to argue that whilst this certainly seems to *tend* to be the case, there are many exceptions and these claims can only really be made as localized and context-dependent claims (ibid).

To deny the ID thesis is not automatically to downplay the significance of computer simulations; this would only be the case if one is to both assert the ID thesis and then subsequently go on to assert the ED thesis. Winsberg is one such philosopher who rejects the ID thesis (2009) but is a firm advocate of the importance of computer simulations. In his (2019) he rejects the ED thesis through a combination of downplaying the automatic and universal significance of experiment, uplaying the significance of simulation, and picking apart differences and similarities between the two, especially with regards to representation and methodology.

A position that denies the ID thesis with the intention of downplaying the significance of computer simulations is that of Gilbert and Troitzsch (1999). They write of the difference between computer simulations and normal experiments as being that 'while in an experiment, one is controlling the actual object of interest... in a simulation one is experimenting with a model rather than the phenomenon itself' (Gilbert and Troitzsch, 1999, p13). This view has been (rightly, I think) criticised by various philosophers (Morgan, 2003)(Parker, 2009)(Winsberg, 2009). Winsberg (2019) points to Peschard's (2010a) defence of Gilbert and Troitzsch — and thus rejection of the ID thesis with the intention to downplay the significance of computer simulations — as a more sophisticated and nuanced position that is similar in essence to Gilbert and Troitzsch.

5.3 Non-Empirical Confirmation: Dawid

In his (2013), Dawid uses the case study of string theory to argue that a considerable degree of trust in an empirically unconfirmed theory — that is, empirically unconfirmed within its specific domain — can be generated based on forms of non-empirical theory confirmation. In his (2019), he provides a general conceptual framework for this framework of such confirmation.

First, a brief discussion of non-empirical theory *assessment* is touched on. The difference between assessment and confirmation being that assessment is the process of the scientist choosing which project that they will work on. Dawid makes a questionable assumptions at the beginning of his (2019), which seems somewhat foundational to the project:

'Reducing what is at stake in non-empirical theory assessment to the question of justifying work on a theory seems at variance with the main motivation for doing fundamental physics. Fundamental physics today clearly is not driven by the perspectives of technological utilization, but rather by the quest for acquiring knowledge about the world'.

(Dawid, 2019, p103).

Motivation, which Dawid seems to use synonymously with "driven by", can be understood in one of two ways. We can understood it internally or externally; that is, motivations internal to the discipline of physics, or motivations external to the discipline of physics. Regarding the internal aims, we can divide this further into the motivations of the individual and the individuals of the group, as discussed in the previous chapter. It does not seem clear to me at all that the motivation of the individual scientist is definitively about the question for acquiring knowledge about the world. This surely needs a sociological survey of some sorts to assert such a claim. The claim that physics, as a community, is motivated by the quest for acquiring knowledge about the world is one that is at odds with van Fraassen's claim that the aim of science is empirical adequacy. We are left at a seeming impasse.

There are good reasons to think that physics is also driven by external factors and motivations. A particular case of this can be seen in the U.S, with military funding towards physics with the expectation that the physics departments develop better technology for the U.S military. Forman (1987) goes into depth about how, towards the end of world war two, each branch of the U.S military made assessments of different areas of scientific research for America's war effort and for the future, with the conclusion being that partnership btween the military and scientists is essential. Military funding in the late 1940s meant that institutions like MIT emerged with staff twice as large as pre WW2, a budget four times as large, and a research budget ten times as large (Forman, 1987, p155-156). The Korean war in the 1950s let to military funding doubling in size for various departments (ibid, p157). In 1950 in the U.S, 70% of all research time of university physicists was spent on studies by the DOD (department of defence) or AEC (atomic energy commission), and 98% of the $22 million that the government used on academic physics came from the DOD or AEC (Kragh, 1999, p295). Kragh writes: 'through the first two decades after the war, the dominant sources of funding for American physics were all related to the military system' (ibid, p296). As for what consequence this had on physics: this period of U.S physics, and arguably still now, was and is dominated by "shut up and calculate" approaches to physics (Schweber, 1994). The Military's desire for applied physics led to a huge increase in these fields. Nuclear physics experienced huge booms in numbers of people working on it and in funding, as did solid state physics and other applied fields. Forman writes that American physics underwent a 'qualitative change in purpose and character' (Forman, 1987, p150), and Schweber (1989) argues that WW2 changed that character of science in that it changed the relationship of science to the military (Schweber, 1989, p670).

The point of the above paragraph is that external drives of disciplines of science do exist, and thus there is good reason to think that what drives a particular discipline is not exclusively internal motivations, but external ones also.

Dawid wants to understand confirmation in a Bayesian sense, whereby some new evidence will update the probability of such a theory being true. He makes clear that there can never be non-empirical evidence so strong that a theory can be conclusively confirmed, and that '[s]trong empirical testing therefore must be expected to remain the only path toward conclusive confirmation of a theory in fundamental physics' (2019, p108). But non-empirical evidence can, he argues, nonetheless lead to a substantial increase in the probability for a theory's viability (ibid). What these pieces of evidence are differ from typical theoretical virtues that have been historically discussed in the literature such as simplicity, ontological parsimony, elegance, etc. (ibid, p109); the latter, it is argued, are too subjective and hard to quantify properly. Instead, he gives three conditions:

(1) Based on observations about the external world.

(2) There exists a meta-level hypothesis, Y, which can be “softly” empirically confirmed by some non-empirical evidence. This, taken together with a positive correlation between Y and the viability of the theory, T, establishes that the non-empirical evidence confirms T.

(3) Viability rather than truth. In other words, non-empirical confirmation should be applicable to theories that make empirical predictions.

(ibid, p113).

I take (1) and (3) to be relatively straightforward and not in need of further exposition. (2) is slightly more ambiguous, but will be explicated in sufficient depth in a couple of paragraphs time.

He presents three arguments that he claims satisfy these conditions:

NAA (The no-alternatives argument): If scientists have spent considerable time looking for other theories and yet have not found any, then this is taken as confirmation of the existent theory (ibid, p114)

MIA (The meta-inductive argument from success in the research field). In the history of the research field in question, theories that satisfy certain criteria have tended to be viable theories. Therefore, if a current theory satisfies these same criteria then it confirms the current theory (ibid).

UEA (The argument of unexpected explanatory interconnections). If a theory, T, is developed to solve a specific problem and it also provides explanations for various other problems for which it was not developed, then this confirms the theory in question (ibid).

All are concerned not with the theory’s truth but with the theory’s viability, thus satisfying condition (3). All three are also concerned with empirical evidence of some sort, thus satisfying criterion (1). Regarding condition (2), the meta-level hypothesis that Dawid is concerned with to practically relate the three arguments to this condition is that of underdetermination. This is discussed in more depth in his (2013), but summarised in his (2019). Recall that condition (2) has two components: (i) that there exists a meta-level hypothesis, Y, that can be “softly” empirically confirmed by non-empirical evidence (E); and that (ii) when we take this fact together with a positive correlation between this meta-level hypothesis, Y, and the viability of T, this establishes that E confirms T. This is quite dense, so is helpful to unpack.

Starting with (i): the meta-level hypothesis, Y, that Dawid is concerned with is that of the limitation of underdetermination of the actual — not potential — empirical evidence for different scientific theories (2019, p114-115)(2013, p50-53). By (i), what is meant is that if Y occurs, then it will produce various effects that we can look for and "empirically" confirm in some very loose sense of the term. Regarding (ii), the positive correlation between Y and the viability of the theory in question is seemingly uncontroversial: the more you can be sure of the fact that the theory in question is *not* underdetermined — the more you limit the prospect of underdetermination — then the higher the viability of the theory in question. This positive correlation, taken together with the fact that the predictions that Y makes are confirmed, establish that E confirms T.

So what's left is for Dawid to show how the NAA, the MIA, and the UEA are all "predicted" in some sense by Y (by limiting underdetermination). The NAA is the most straightforward. Start with the most extreme example — if there is maximum limiting to underdetermination, then there is no underdetermination, and thus Y would predict that there are no conceivable alternatives to T. To use this as a practical example to illustrate Dawid's framework here: Y predicts that there are no alternatives to T. Scientists cannot find any alternatives to T, thus "softly" confirming Y. This fact, coupled with the positive correlation between Y and T, confirms T.

The MIA example is claimed to satisfy (2) because if there are no or few alternatives to the theories in question, 'one would expect a fairly high percentage of those theories to be viable' (2019, p116). The UEA case employs the following reasoning. If there were more viable theories (therefore a higher probability of underdetermination occurring), then there would be a higher likelihood of one particular theory explaining fewer problems; if there were less viable theories (therefore a lower probability of underdetermination occurring), then there would be a much lower likelihood of one particular theory explaining fewer problems. At the most extreme case, if there is no underdetermination conceivable and we have just one theory that we are prepared to consider, then the prediction from this is that this theory T will explain all of the problems that we are solvable (ibid).

A very important clarification to all of this is that the arguments — NAA, MIA, UEA — should be taken as confirmatory *not* in isolation, but only when they can be made *together*, either with two of them or, ideally, all three together (2019, p117). For instance, the NAA argument by itself can be explained by an arguably simpler meta-level hypothesis: that the scientists in this field are simply failing to find the theory through whatever reason (inadequacy, looking in the wrong place, working with the wrong conceptual framework, etc). But when the NAA argument is taken in conjunction with the UEA argument, and thus the theory has both no found alternatives *and* has been found to be unexpectedly explanatory, then this does seem to work better with the limiting underdetermination hypothesis, Y, as opposed to the "inadequacy of scientists" hypothesis. And thus does seem to be confirmatory.

5.4 The Compatibility of Methodological Empiricism with Analogue Experiments, Computer Simulations, and Non-Empirical Theory Confirmation

In this section I examine the compatibility of ME with analogue experiments, computer simulations, and NEC. The position of ME that I use is the position that was developed in the previous chapter, which I claim is the most suitable version of ME to hold in light of current science.

Before comparing these positions directly to ME, I first argue that all three positions are dependent on an empirical foundation in a significant way, and that this thus matches the most basic of the ME characteristics of "empirical investigation". This was foundational to the development of the ME position in chapter four, and without it is hard to see how these positions could be compatible with ME. In fact, demonstrating that they match the four criterion set out and yet aren't developed on solid empirical foundations would signal a problem rather than a positive. If a position that is detached from empirical investigation can match four criteria that are supposed to be criteria for an empiricist position with an empirical methodology at its core, then something has gone wrong along the way.

Recall from the last chapter the prescriptive criteria that were given for empiricism: (1) it must allow for the collective nature of science; (2) it should embrace the shift that has occurred in philosophy of science towards an increased focus on scientific practice as opposed to predominantly holding a focus towards theories; (3) it should not give epistemic privilege to what is observable by the naked eye, but should instead epistemically privilege the class of "measurables"; and (4) it should hold a position of causal realism. It is this formulation of empiricism — a specific brand of empiricism that exists within the ME version — that will be used in assessing whether or not simulation, analogue experiments, and Dawid's conception of non-empirical theory confirmation prove problematic or even damning for empiricism.

5.4.1 Empirical Foundations

Computer Simulations

Computer simulations are grounded on empirical evidence, and the epistemic warrant that we get from them is warranted insofar as it is empirical. To this point the concept of "validation" should be turned to. This was touched upon very briefly in (5.2). Validation is a technical term and denotes the process that scientists undergo to ensure that the simulation is accurate with respect to the target system that it is trying to represent in some way. Various different technical definitions have been proposed for precisely what this is. A recent popular definition is from the American Institute of Aeronautics and Astronautics (AIAA):

'The process of determining the degree to which a model is an accurate representation of the real world from the perspective of the intended uses of the model'

(requoted from Beisbart, 2019, p40).

This is the definition accepted and adopted by Oberkampf and Roy in their (2010) monograph on the topic of validation. But a slightly more relaxed and pragmatic version of the term is given by Murray-Smith (2019). For him, validation is:

'used to describe procedures for establishing whether the model fidelity is adequate for the purposes of the given application... validation processes involve information external to the model, normally using data or observations from the corresponding real system'.

(Murray-Smith, 2019, p99)

There are various other ways which one can define this term (See Beisbart, 2019). But there is no dispute that validation plays an extremely important role in computer simulations, such that it is the basis of forming epistemic trust in any particular simulation. If the computer simulation cannot be said to be accurate to some degree with respect to the system that it is aiming to simulate — the target system — then it cannot be trusted, epistemically speaking. The way in which validation takes place varies among sciences. For instance, Oberkampf and Roy (2010), and Oberkampf (2019) present models of validation more specific to the physical sciences; these accounts are more stringent and require reliance on data from physical experiments that make them not really applicable to the social sciences.

But validation is, in practice, an activity that is driven by a desiderata of ensuring that the outputs from the simulation correspond as closely as possible to the *measured* data, and thus renders this activity thoroughly empiricist in nature. Beisbart (2019), in an article designed as an overview and survey of the various different accounts of validation in the literature, writes that:

'whatever we think about the possibility of a form of validation that is not data-driven, it is true that, in practice, the comparison between simulation output and measured data is most often absolutely needed for validation'

(Beisbart, 2019, p63).

And, as per (4.3), it is practice that trumps theory and philosophical speculation on the matter.

The point of all this is that computer simulations are trusted only insofar as they can reliably be demonstrated to accurately correspond to the target system — to satisfy a process of validation. Validation is an empirical process, with measurement at its core, thus satisfy some sort of criterion of "empiricalness". The scientific community places significant epistemic trust in computer simulations *because* they can be shown to be accurate in this important sense.

Parker (2008) argues that the process of validation in computer simulations can draw heavily on processes that exist in more traditional forms of experiment. To this end she turns to Frankin's (1986)(1989)(2002) work on the epistemology of experiments, and systematically goes through his criteria by which experiments are seen to be epistemically secure. She notes five important parallels that exist in computer simulations:

(1) Demonstrate that simulation output other than that whose trustworthiness is at issue fits will with the empirical data that are available for the target system. (ibid, p169)

(2) Demonstrating that changes to the value of important model parameters affect the simulation output in ways that are expected, given what we already empirically know about the target system. (ibid)

(3) Confidence inspired by pointing out that key modelling assumptions come from accepted scientific theories. (ibid, p170)

(4) Shows that a simulation result whose external validity is at issue closely matches a result generated in another study which addresses the same question about the target system. (ibid, p171)

(5) Ruling out of error due to factors such as simplifications and idealizations. (ibid, p172)

Of these, none are non-empirical in a way that would threaten this project, and three of these — (1),(2), and (4) — are explicitly empirical. (3) relies on modelling assumptions in computer simulations being more epistemically secure if they are founded on more secure and well-trusted scientific theories; but scientific theories are trusted largely because they have been empirically well-tested. (5) can arguably be said to be explicitly empirical also: various possible errors that arise due to processes of simplifications and idealizations are checked against the available data that we have for the target system.

Putting aside the topic of validation, there are several other authors who explicitly claim that the epistemic trust that we place in computer simulations arises because of empirical means. Symons and Alvarado (2019) argue for the normative claim that the trust that we place in computer simulations should be grounded in 'empirical evidence, good engineering practice, and established theoretical principles' (ibid, p37). Under this view, simulations in and of themselves are 'empty' (ibid, p40), and they conceive of simulations as being a specific category of scientific instruments. They are potentially extremely useful tools, in the right hands, but must be used in such a way by those with the relevant expertise and following the correct, clearly empirical principles and justificatory methods (ibid, p52). Alvarado (2021) expands much further on this claim.

Others who argue for the trustworthiness of computer simulations explain this epistemic trust via empirical data. For instance, Parke (2014) writes:

CHAPTER 5. "NON-EMPIRICAL" APPROACHES IN PHILOSOPHY OF SCIENCE: A THREAT TO METHODOLOGICAL EMPIRICISM?

'We know how to do good computer simulations precisely because we have gained knowledge about the world through observation and experiment' (p518)

'Empirical data are fundamental for answering scientific questions about the natural world' (ibid).

Analogue Confirmation and Experiments

There are two ways to approach this question, here. The first is to keep a more orthodox understanding of what "empirical" means, and to analyse analogue confirmation around this, imposing conditions on what can and cannot be done within the realsm of analogue confirmation, all restricted within an empirical framework using this orthodox understanding of empirical. The second way is to modify the understanding slightly of "empirical", and to argue that analogue confirmation is empirical if we are willing to understand the latter in this broader sense. The first strategy is employed by Field (forthcoming), and the latter strategy, in some form, is employed by Evans and Thébault (2020). I will outline both above, with the conclusion being made that it is not of vital importance whichever way one chooses to proceed; the main point here is that they can be shown to be based on solid empirical foundations.

Field

As mentioned in (5.1), universality arguments are foundational to analogue confirmation. The way in which we would be able to make a successful inference from the results of the analogue experiment — the source — to the target system is via an appeal to a universality argument. Recall also that Field (forthcoming) gives two criteria of success for universality arguments. Either (1) we have a good knowledge of the microscopic structure of the system from which we are appealing to universalise (ibid, p21), or (2) we have empirical access to the macroscopic behaviour of the system from which we are appealing to universalise (ibid, p22). Or both.

Field directly compares Wilsonian renormalization-group universality arguments in condensed matter physics — which are extremely successful — with Hawking radiation universality arguments — which we do not know if they are successful — to derive these criteria. The former explain why condensed matter systems that are microscopically diverse such as liquids and ferromagnets 'exhibit the same critical behaviour in the region of a continuous phase transition' (ibid, p20). In practice this argument succeeds because we have sufficient knowledge of the microscopic structure. As Field puts it, it succeeds because:

'we know enough about the microscopic structure underlying condensed matter physics to be confident that the systems of interest do share the features that the argument invokes. In particular, we can be confident that a vast array of systems in condensed matter physics, which superficially seem to be very different systems, do share the feature that they can all be described as lattices of a particular dimension d and order paramater n at the microscopic level.'

(ibid, p21).

As was discussed in the previous subsection, to have reliable knowledge of a domain — of the microscopic structure of a system — holds as a prerequisite that we have been able to empirically test this in some way. To have knowledge of microscopic structure of a system requires us to have strong empirical evidence for this system.

Criterion (2) is, in the case of renormalization-group universality arguments, a counter-factual of sorts. *If* we didn't actually have good knowledge of this microscopic structure, we would nonetheless be able to have a good universality argument here if we could empirically access the macroscopic behaviour of the condensed matter systems. The universality argument could be framed as a hypothesis and its consequences could be tested. This is clear to see just how this is suitable for an empiricist framework: we here directly rely on empirical testing.

To stress this point more, Field writes that the tools in this framework are:

'essential as a reminder of how our theoretical tools are shaped by our empirical capabilities... if we cannot empirically access the macro-behaviour of the physical system in question, and if we do not already know that they share the relevant micro-structure, then no theoretical argument — not even a universality argument — can fill the empirical gap'

(ibid, p3).

If, as per Field, universality arguments require wholly empirical criteria of success in order to succeed, and successful universality arguments are a prerequisite in order for analogue confirmation to be successful, then this renders any successful analogue confirmation as empirical in nature. This therefore poses no threat to the empiricism that I have laid out.

Evans and Thébault

The alternative way to argue that analogue confirmation is empirical in some sense that is compatible with empiricism is to modify what is traditionally understood as empirical. This is akin to the strategy of Evans and Thébault (2020), who claim that the limits of experimental knowledge is set by inductive triangulation rather than the more orthodox criteria of manipulability, observability, and accessibility. Inductive triangulation uses several different inferences that use different uniformity principles, as was briefly stated in (5.1). These are combined in scientific practice to gain knowledge of phenomena.

An example that they give is of a justification of an ampliative inference from iron atoms in the source system to iron atoms in the target system; that is, a justification that the iron atoms in both the source and the target will be similar enough to be able to warrant a belief that what holds for one will hold for another. The type of uniformity principle that we immediately hold for this justification is that of *intra-type uniformity*: we can license the inference between one group to the other because of the fact that they are of the same type, or kind, or however we want to phrase this. But this of course is circular in the way that Hume showed — it effectively breaks down in structure to "we can believe that the atoms in A are the same as the atoms in B because they are the same". So we can introduce more uniformity principles in order to "triangulate" the inference. In the atoms example, we can introduce spatial uniformity and temporal uniformity to explain why the inference is justified. The explanation of why we are licensed in our inference of similarity then becomes: "we can believe that the atoms in A are the same as the atoms in B because they historically always have been when it comes to measuring them (temporal uniformity), and because there has never been a difference in properties of atoms measured further away than closer to us (spatial uniformity)". They thus claim that there is far more justificational power here (ibid, p8-9).

Two case-studies are given as evidence for this type of reasoning occuring in scientific practice. The first is in the model of stellar nucleosynthesis (ibid, p10-12), and the second is Hawking radiation in analogue black holes (ibid, p 12-18). The first case study of stellar nucleosynthesis (ibid, p10-12) and is sufficient to demonstrate what is meant here. It involves well-established science that involves a target system that is unmanipulable, unobservable (in the philosophical sense), and inaccessible. Constraints that arise which lead to this unmanipulability, unobservability, and inaccessibility are overcome via inductive triangulation. Inferences about the target system are 'cross-referenced to terrestrially observed spectra' (ibid, p11), and validated (in the technical sense that we see in (5.4.1)) via a 'range of independently established theories' (ibid). With this particular case, intra-type uniformity is at the core of it (ibid), as well as spatial and temporal uniformity principles.

Thébault says that this approach fits into a liberal form of empiricism (2021). He characterises this as being:

'a form of empiricism within which the scope of phenomena about which we can gain experimental knowledge is much wider than that of [...] restricted forms of empiricism'

(Bristol Centre for Science and Philosophy, 2021, 23.18)

Non-Empirical Theory Confirmation

As was seen in (5.3), Dawid makes absolutely clear that theories should not be confirmed by values such as those we would typically associate with theoretical virtues (simplicity, ontological parsimony, elegance, etc). We see also in (5.3) that Dawid gives three conditions that must be satisfied in order to qualify as being confirmatory of theories. Two of these are directly concerned with being empirical. The first is that they must be based on observations about the external world, and the other is that they must be concerned with viability as opposed to "truth" — they should be applicable to theories that make empirical predictions. And the three arguments that he presents — the no-alternatives argument, the meta-inductive argument, and the argument of unexpected explanatory interconnections — are, Dawid says, all concerned with empirical evidence, and are thus based on observations about the external world. They are also all concerned with the theory's viability. And elsewhere Dawid explicitly stresses that NEC is 'based on observational evidence' (Dawid, 2019, p100). The "non-empirical" component of NEC refers not to the claim that the confirmation/evidence is not empirical, but instead that it is not in the specific theory's domain.

NEC is self-consciously working within less-than-ideal constraints. It is acknowledged that empirical evidence is the gold standard of confirmation of scientific theories, but points to modern physics as requiring some sort of modification to its method of confirmation if we want to make advances in fundamental physics. NEC 'does not affect the role of empirical data as the ultimate judge of a theory's viability' (Dawid, 2013, p2). Thus, the high prestige given to empirical evidence is not diminished in this programme, it is simply a programme that seeks to find alternatives to confirm theories than empirical evidence within a theory's given domain, whilst nonetheless still heavily appealing to an empirical foundation.

5.4.2 Criteria of Methodological Empiricism Compared

Collective, Not Individual

There are occassions where NEC will be framed in terms of an individualistic epistemology. For instance, in Dawid et al (2015) the "no-alternatives" argument is given a formal treatment using a subjective Bayesian framework. They talk here in terms of 'subjective degrees of belief' (ibid, 215), the 'subjective degrees of belief of a scientist' (ibid, p222), and about the 'scientists' subjective judgements' (ibid, p214). In fact, in this paper one of the crucial aims is to show that the significance of the "no-alternatives" argument in scientific reasoning depends on these subjective judgements of the scientists (ibid). This is qualified, though, with the acknowledgement that the fact that the assessment of this will differ from scientist to scientist is 'unfortunate' (ibid, p222), and seeks to remedy this is in the subsequent part of the paper by appealing to direct empirical evidence to give information about the probable number of alternatives to a theory.

In his (2013) he seems to explicitly acknowledge the limits of framing things in this more individualistic manner. He focuses on Bayesian theory confirmation, and notes that whilst the probabilities of the theory have an entirely subjective character, they are not that reliable. 'Only then do the probabilities that initially have an entirely subject character, and may be chosen quite differently by individual scientists, converge and thus start constituting reliable scientific judgements' (2013, p41). Dawid here also generally stresses the importance of science-as-a-community, citing the remarkable development of the LHC, the number of physicists needed to work on the experiments, and how this simply cannot be carried out by an individual researcher (ibid, p76).

He is also critical — rightly so — of what individual scientists have believed, in terms of theory assessments and how this fares for science. [4]. Further, he is critical of more purely theoretical considerations of theories, such as 'simplicity, beauty, or apparent cogency' (ibid, p42) because of the fact that they are too subjective, and not an objective and independent factor (ibid). There thus seems to be something of a tension between the occassions in which Dawid uses a subjective Bayesian epistemology, which is *prima facie* an epistemology of the individual, and his insistence on the importance of the collective. NEC should certainly be seen as compatible with this collectivist epistemic focus that I have stressed as an important component of ME, but as it currently stands with Dawid's formulation this is not properly instantiated.

Within the philosophy of computer simulation literature, there is a focus amongst some on the epistemology of computer simulation (Winsberg, 1999)(Winsberg, 2001). There is no discussion that I have found on this epistemology taking a specifically individualistic slant; and nor is there a focus on a collectivist epistemology of the sort advocated in (4.2). The discussion is generally framed far more generally. But as per NEC, there is nothing incompatible within the literature that would prohibit the introduction of a collectively focused epistemology as per (4.2). This is also the case within the literature of analogue experiments and confirmation. There is certainly here a central focus on epistemology — namely via the claim that analogue experiments can 'provide a suitable form of external validation' (Thébault, 2019, p189), which should warrant our beliefs in trusting this experiment and thus believing that the theory in question is "confirmed". But epistemology is discussed in very general terms, and it is not stated whether one should or should not embrace a collectivist or individualistic epistemology. And just as per the previous two fields, there is nothing here that would indicate that the literature on analogue confirmation and experiments is incompatible with a collectivist epistemology as was highlighted in (4.2).

[4] See: 'Despite strong cases of the above kind [an example of Einstein's assessment of the probability of general relativity being correct], however, the canonical paradigm of theory assessment understands such considerations as subjective assessments which do not generate genuine scientific knowledge about the external world. Earlier stages of this book have suggested that this canonical paradigm of theory assessment is seriously at odds with the way empirically unconfirmed theories and claims are actually assessed in contemporary fundamental physics... it will be demonstrated that this is true... also within the context of well-established standard model physics' (Dawid, 2013, p104)

Practice-led

Given that all three of these positions are directly based on relatively new developments in science, and thus focus directly on actual scientific practice and applying philosophy to these direct case studies, it is evident that the focus is on scientific practice rather than theories taken in isolation. Given that they all rely on case studies and direct instances of scientific practice, they are also focusing on what works in practice over what works in theory.

However, the criterion of requiring a position be practice-led is in reference to well-established science. Computer simulations are surely well-established; analogue confirmation is something of a contested topic within science, and more of a fringe topic than is computer simulations. For instance, Field (forthcoming) quotes Harlow as arguing that analogue experiments are 'an amusing feat of engineering that won't tell us anything about black holes' (Field, fortchoming, p8). The fact that there is even one strong dissenter demonstrates that this is not firmly established practice — it is impossible to imagine that any practising physicist would be making such claims about such strongly established fields and practices as fluid mechanics, traditional experiments, telescopic observations, or condensed matter physics.

The field that NETC relies most heavily on — string theory — is even more heavily contested, both in claims that it is the correct candidate for a theory of quantum gravity, and even on its seemingly non-empirical methodology. This will be elaborated on in (5.4.3), where I will argue that we should prioritise ME over NETC.

To stress the point: all three fields are practice-led, and so satisfy this criterion of ME. But — to anticipate an argument to be made — where the scientific practice that it appeals to is contested practice, this does not hold the same merits as firmly established practice. The more secure the scientific practice, the more we should trust it; the less secure the scientific practice, the less we should trust it.

Measurement, not Observability

In the philosophy of computer simulation, there are a couple of authors who have performed research on measurement in computer simulations, and how the two relate. These are Parker (2017) and Morrison (2009). Both argue that computer simulations are used in the process of measurement. In Parker's case, it is argued that computer simulations are often embedded in the process of measurement; in Morrison's case, it is argued that computer simulations can be used to perform measurements. For present purposes, what should be of concern here is to what extent the literature *does* and *can* epistemically privilege what is measurable. Parker's (2017) research does not apply here. The crux of her argument is that computer simulations, on their own, are 'not a process for measuring properties of the system being simulated' (ibid, p285); but that they can be embedded in measurement practices in important ways. Computer simulations being involved in the process of measurement as a tool of some sort is not the same as computer simulation epistemically privileging what is measurable.

Morrison (2009) argues that we should treat certain types of computer simulations as having the same epistemic status as measurements made via standard experimentation. Her strategy of arguing this is twofold. First, it is argued that models play an important role in measurement in experiments, and that models can in some important way be said to measure (ibid, p35), playing the role of a measuring instrument. Second, it is argued that computer simulations are similar enough to experiments that the same reasoning can be transferred across. Both of these claims should be contested here.

Regarding the first stage: Morrison points to an example of the physical plane pendulum that can be used to measure local gravitational acceleration. We make the measurements using this device, but a large number of corrections need to be made in order for this measurement to be accurate (2009, p49).[5] Some of these corrections involve complex applications of models, and thus there is extensive use of theoretical models within this measurement such that it would not be able to be said to perform a measurement without these models — 'the ability of the physical pendulum to function as a measuring instrument is completely dependent on the presence of these models' (ibid, p35). It is then in this sense, she argues, that models function as measuring instruments (ibid). Summarised in her words: '[w]ithout models, there is no measurement' (ibid, p50).

It is certainly true that measurement requires models in this sense, and that they form a necessary condition in the overall process of making a measurement. But simply because these various models are necessary in order for

[5] These include making (i) finite amplitude corrections, (ii) mass distribution corrections, (iii) effects of air including buoyancy, damping, added mass, and theoretical damping constants, and (iv) elastic corrections due to wire stretching (Morrison, 2009, p49).

the measuring device — the pendulum — to be properly used, *this does not mean that the models are doing the measuring.* The models cannot be said to be performing a measurement. Closely analogously: scientific theories are absolutely essential to be able to make measurements in current science; but surely no-one would claim that theories are performing a measurement, or are doing the measurement. In essence, the claim that Morrison is making here seems to be that if x is a necessary condition for y, and is thus absolutely vital for y's existence, then this functions as equivalent to an identity statement — that x is y, or that x is a subset of y. This is evidently not true. Based on this line of reasoning, we should not accept her claim that 'models function as "measuring instruments"' (ibid, p33).

The second stage depends for its soundness on this first claim, and thus if the first claim is rejected then the second claim has no bite. But it is worth addressing this second point, both in case one is not convinced by my claims made regarding Morrison's first argument, and given that this is the stage that is of greater importance to the issue at hand here. In her second main claim, she argues that computer simulations are similar to experiments in that they are both given epistemic justification via the models that they use (ibid, p43); and that in both computer simulations and experiments models 'function as the primary source of knowledge' (ibid) in such a way that we should assume them to be methodologically and epistemically similar enough for this comparison. Thus, what is doing the philosophical heavy-lifting, and is foundational to both, is models. Providing our models are sufficiently chosen and generally fit for purpose, a computer simulation can be said to perform a measurement — to function as a measuring instrument — in the same way that experiments do in virtue of both being reliant on models.

Computer simulations evidently do perform some sort of measurement, insofar as when the simulations are performed, a measurement is taken of the simulation in question. But this is a measurement of the source system, and not the target system; yet it is the target system that is epistemically privileged. Thus, any account that seeks to epistemically privilege measurement in computer simulation must make a solid case that the epistemic privilege that we derive from the measurement applies to the target system (the system that the computer simulation represents), and not the source system (the computer simulation itself).

Morrison does this. She argues that whilst in the computer simulation it's technically true that it is the model under investigation, and not the target system itself, it is claimed that the model is serving as a *representation of* a physical system and thus should be able to be treated as an accurate account of the target system, providing the model is suitable (ibid, p45). This is clearly entirely dependent on the model being a sufficiently accurate representation of the target system; and the way that we would verify that the model is a sufficiently accurate representation of the target system is via empirical means. This is the process of validation, as has been discussed earlier. Thus, the model used in the computer simulation only has epistemic strength insofar as it is a sufficiently suitable representation of the target system, which, in turn, is only a suitably sufficient representation judged entirely by empirical standards, and this process ultimately rests on the degree to which the simulation can be validated against its target system. It therefore seems that it can be said that computer simulations can and do satisfy this criterion presented in ME.

Regarding analogue confirmation, it is absolutely clear that they move away from the epistemic privileging of observable, understanding observable as sensory perception. In analogue experiments and confirmation there is nothing in the literature explicitly discussing measurements and how these tie in to the overall project. But, just as is the case for the criterion of collectivist epistemology, there is similarly nothing to suggest that this is a position that is incompatible with this criterion of epistemically privileging measurement. It is clear that when performing the analogue experiment one takes a measurement of the phenomena that is being experimented on. However, as per in computer simulations, what differs is that the direct phenomena that is being measured is not the phenomena that epistemic privilege is claimed for. What *is* being claimed that we should epistemically privilege is the target system, not the source system. This initially seems problematic when deciding on the compatibility of this with the criterion developed in (4.4). But turning to Evans and Thébault's (2020) work provides a way out of this problem. They extensively discuss how there are a plethora of ampliative inferences performed in every experiment in physics, and that the inferences that we make from source to target system in analogue experiments is simply a special instance of the normal procedure of inferences used in experiments, which is made epistemically secure by "inductive triangulation". Even in the most orthodox, secure experiments, we are making ampliative inferences in assuming that the *particular* system that we have experimented on and measured can be generalised to *every* system of the same kind.

NEC is not concerned with measurement, and it is hard to see how measurement would play a central role in this framework. Whilst, as stated in (5.4.1), Dawid implements two empirical conditions that must be satisfied, they are to do broadly with observation of the external world, and secondly that the framework should concern theories that make empirical predictions. The type of observation that he is concerned with is not explicitly measurement-focused, and the spirit of the whole project of NEC is that of stepping away from a measurement-focused empirical approach and towards one where other means can be considered. There is a certainly a desire in NEC — as shown above in (5.4.1) — for an empirical foundation of some sorts, but this empirical foundation is not concerned with measurement. The NAA argues that if scientists have spend considerable time looking for other theories and have not found any, then this confirms the theory in question (2019, p114).

One may thus object that there is a measurement of sort taking place — one measures the scientific community to decide whether or not the NAA has been met; and a measurement that is sufficient allows us to confirm said theory. Any sort of measuring that would take place here is not the sort of measurement that is relevant here. It would not be measuring the world in a material sense, but measuring the community of scientists and time taken looking for theories in a more informal sense. The measurement that could hypothetically be performed here is not one that interacts with the world in a concrete way to gather information about the system, but is measuring something tangential. We should bare in mind the nature of what the measurement is supposed to tell us, according to (4.4). The epistemic privilege that ME argues should be given is of the phenomena that is being measured. Here, what we would be epistemically privileging is the existence of the scientists, since they are what is being measured. This is evidently not the phenomena or area of interest that Dawid wants to point to. The inability to easily quantify the parameters of the NAA also make this harder to perform any sort of measurement on — for instance, what constitutes a "long enough" time? What metric do we use to constitute how "hard" scientists have been searching for an alternative?

The MIA argument argues that in the history of the research field in question, theories that satisfy certain criteria have tended to be viable theories. Therefore, if a current theory satisfies these same criteria then it confirms the current theory (ibid). There is no measurement that could plausibly take place here that would allow this argument to fit into a position that prioritises measurement; it is a philosophical argument that relies on a specific process of inferential reasoning. Similarly, there is no plausible process of measurement that could be invoked for UEA. Here, if a theory, T, is developed to solve a specific problem and it also provides explanations for various other problems for which it was not developed, then this confirms the theory in question (ibid). This is a philosophical argument that, again, relies on a specific process of inferential reasoning, and not on a measurement of any kind. Consequent on these points, there is no way that NEC can be said to allow for prioritising measurement over observation, as per the critierion developed in (4.4).

Causal Realism

When, in (4.5), the criterion of causal realism was presented, it was argued that empiricists should embrace some sort of realism about causality, and this was justified on the grounds that a realist position about causality makes the best sense of the fact that there evidently exists something like causality in the world, and that science frequently discusses and posits causality in its investigations. In analogue experiments/analogue confirmation, and computer simulations, there is seemingly not a causal connection between the experiment and the target system, or between the measuring device and the target system. The system that is studied here is the source system, from which, through various criteria and procedures, one hopes to learn about the target system in some way. In analogue experiments, there is a causal connection between the source and the measurement device, whilst in computer simulations this is more contentious. The disparity between the target system and source system was pointed to in the previous discussion regarding measurement and (i) computer simulations and (ii) analogue confirmation. That is to say, the fact that in both computer simulations and analogue experiments one studies a system that is different to the system that one wants to learn about.

Contra computer simulations seemingly not involving causality, Massimi and Bhimji (2015) argue that computer simulations can at times be shown to satisfy what they call the "causal interaction claim" (ibid, p73). They point to the use of computer simulations in the discovery of the Higgs Boson, and argue that the use of computer simulations involve direct causal interactions with the target system (p79). If they are correct, then this proves additional evidence that computer simulations are not incompatible with ME. But even if one takes issue with this claim, then there is nonetheless still no incompatibility with this criterion of causal realism and analogue confirmation/computer simulations: they could still embrace some sort of realism about causality, despite there being no causal connection between the target system and their particular experiments. They may very well believe and argue that causality exists, but simply that a causal connection is not necessary for analogue confirmation and/or computer simulations to be significant. Adopting a position of causal realism is not the same as arguing that causality has to exist ubiquitously for various parts of scientific practice to have significance. We cannot causally interact with black holes, dinosaurs, or the earth's core, and yet it is true that we have knowledge of all three of them. To be implementing a form of empiricism that aims to be compatible with current science simply requires us to grant knowledge of these sorts of phenomena.

The same is true of NEC. Advocates of NEC could still clearly embrace causal realism, and seem to be better placed to make claims about causality existing within their proposed frameworks than do those in analogue confirmation and computer simulation.

5.4.3 The Incompatibility of Methodological Empiricism and Non-Empirical Theory Confirmation

Taking stock here, it has been argued that analogue confirmation and the philosophy of computer simulation — and the field of computer simulations and analogue experiments more generally — are perfectly compatible with ME, given that both of them satisfy all four criteria that is presented by ME, and both have an empirical foundation that they operate from. There are no fundamental tensions here that cannot be resolved. Whilst there initially seems to be a tension regarding the causality criterion, the point of this criterion is that the only sort of knowledge that we can have of phenomena must be causally connected, but that causal realism should be embraced. We cannot causally interact with black holes, dinosaurs, or the earth's core, and yet it is undeniably true that we have great amounts of knowledge of all three of them.

This compatibility is not true, though, for NETC — there is an irresolvable tension between ME and NETC due to the fact that NETC cannot allow for the criterion of epistemically privileging measurement that has been put forward for ME here. Instead of this being damning for ME, though, I instead argue that it is damning for NETC. Presumably the initial response to this will be an objection: I say that ME should look at the practice of science, yet NETC is evidently drawn from real scientific practice in the form of deriving it's practices from string theory. So how can it at once be claimed as a criterion that one should turn to scientific practice, and yet also here claim that NETC must give way to ME when the two hold different values? Surely if one has to be given up, it is ME?

When insisting that one turn to scientific practice is, there is evidently an assumption that the scientific practice that should be turned to is well-established, accepted science. If ME clashed with, say, quantum electrodynamics, or quantum chromodynamics, or classical mechanics, or general relativity, or even computer simulation practice (which is ubiquitously employed through the sciences) then so much the worse for ME. But string theory does not hold this level of prestige or acceptance in the scientific community. Even the most adamant string theorists would not claim that string theory is accepted as a correct theory for quantum gravity. There are competing research programmes such as loop quantum gravity, shape dynamics, causal set theory, to name a few. Whilst individual scientists surely have their own views of what is right and what is wrong, there is no consensus as to what is the correct theory here within the scientific community.

As well as being objected to on the level of being the correct theory, string theory has also faced great amounts of criticism from other physicists regarding its non-empirical methodology that it seems forced to implement, as Dawid clearly points out and has been seen. If we presume that Dawid is right, and that string theory does use NETC, then this sort of reasoning is nonetheless highly contested. Smolin (2006), Hossenfelder (2018), Woit (2006) are just three physicists who have published popular science books attacking string theory for methodological reasons in that it's not sufficiently empirical. Ellis and Silk (2014) accuse non-empirical methodological proposals such as string as 'moving the goalposts' of scientific inquiry (ibid, p322).

The appeal to scientific practice as a criterion of ME should therefore be qualified with the claim that in instances where the actual scientific practice is contested by other scientists, we should fall back onto the other criterion that have been presented in ME in order to help with our decisions within science. The collective nature of science and causal realism do not pose especially useful guides here, aside from re-asserting the justification in turning to the scientific community to see which theory is accepted and which is not. The criterion of epistemically privileging measurement allows us an extremely productive way to proceed. In this sense, ME can provide an especially useful tool for science in that it can provide a normative criteria that has been derived from an empiricism (ME) that has existed and been developed over centuries. As has already been stated, those who develop ME see ME and scientific methodology as going hand-in-hand, and so it makes complete sense that ME can guide scientific methodology in this manner.

By falling back onto this criterion — demanding some sort of epistemic privilege for measurement — we actually end up in a position that is similar, but more narrow, to Oriti's (2019). He advocates for the "principle of proliferation", which he states as : 'construct as many alternatives as possible to the current (dominant) theoretical framework, and use the set of all such alternative theories (including the currently favoured one) as the object of (empirical) testing, not any given theory in isolation' (Oriti, 2019, p128). I do not have anything to say on this as a more global approach to a methodology of science, but it certainly fits with the more local view presented in this sub-section: keep looking for theories that can be developed that can have a clear empirical component. This comes in the form of measurement, and being able to epistemically privilege measurement.

To give this view more support external to the criteria that were developed in chapter 4, we can turn to Kragh's (2011) analaysis of the very recent history of what he calls 'epistemic shifts' (Kragh, 2011, p360) in science. These are claims from scientists that a new methodological paradigm is needed, or even has begun. In Kragh's own words, they are 'proposals from scientists that traditional criteria of evaluation of scientific theories or practices are no longer adequate and should therefore be replaced by new criteria that better fit the problems under investigation' (ibid). This is precisely what Dawid's NETC framework fits into — a claim that science is moving and must move away from a traditional empirical methodology.

There are some notable historical examples of such claims. Langevin argued that electromagnetism would do such a thing, but ended up using the old well-established methods. Chew, in fundamental physics in the 1960s, argued that the hadronic bootstrap was a precursor to a new science (ibid, p362), and that this method would explain nature because of the fact that it is the only possible option. This is remarkably similar to Dawid's NAA argument, and indeed parts of Chew's methodological thinking was absorbed into the string theory research programme (ibid). Although more extreme than Dawid's claim, there are a couple of examples of scientist's who have proposed that experiments and observations should not be the final say in assessing/confirming theories. Kragh points to Eddington and Milne as two. The former is argued for in cosmology in the 1930s, where it is claimed that various knowledge claims in physics could be made *a priori* and independent of measurement (ibid, p362). Milne constructed a scientific methodology were empirical testing played a similarly neglegible role (ibid). Hossenfelder (2018) also points to various historical examples of physicists who have followed such a non-empirical line of reasoning and ended up nowhere. Most notably, she notes that both Dirac's and Einstein's later years of their career can be characterised by such an attitude, and led to them being staunch objectors of two of the most empirically successful and now most widely accepted physics of all time: quantum electrodynamics and quantum mechanics, respectively.

Yet through all of these, empirical testability has always remained crucial as a necessary condition for scientific practice. Kragh argues that these claims have 'almost always turned out to be more rhetoric than reality' (ibid). This gives us an optimistic conclusion about empirical methodology: that despite claims being made over the last 100 years or so that alternatives to empirical methodologies are near or even here, these claims have not come to fruition. Epistemically privileging measurement thus seems a solid criterion to stick to in cases of contested scientific practice, based on the external considerations of the history of this topic.

Conclusion

In this chapter I have considered three fields of research within recent philosophy of science that *prima facie* are a threat to ME, and to empiricism more generally. These are analogue confirmation, computer simulations, and NETC, and all appear to involve non-empirical elements in significant ways. I argue that the non-empirical component is not as radical as it seems for all of them, and that all do have some sort of empirical foundation. With computer simulations and analogue confirmation, I argue also that there is no irresolvable tension between both of them and the criteria that was given in chapter 4 for ME, and that they thus do not pose a threat to ME. This is not the same, though, for NETC. Here, there is a tension which cannot be resolved insofar as NETC does not and cannot plausibly epistemically privilege what is measurable.

One may reasonably ask, given the insistence on scientific practice that ME requires, why one should favour ME over NETC — which is explicitly based on string theory — given that NETC is based on explicit scientific practice. I argued that the reason was due to string theory being highly contested as scientific practice, both in the fact that it is not established and due to having much scrutiny directed at its methodology. Thus, in areas where scientific practice is up for debate and not established, ME can offer good guidance for what scientists should do by appealing to its other criteria. Here, epistemically privileging measurement is the most suitable criterion.

Conclusion

This project has two aims. The first has two components: to demonstrate that there exists an alternative version of empiricism — ME — to the standard version that philosophers typically associate empiricism with in philosophy of science, and to show that this position has been neglected in philosophy of science. The second aim builds on this first aim, and explored what ME would look like in light of current science. There is also, within this second aim, included a normative dimension — it is claimed that this way of formulating ME is the best way to formulate empiricism in the philosophy of science; and that if one is going to be an empiricist in philosophy of science, then this is the position that one should hold. These aims have broadly corresponded to the two parts of this thesis. The first part of the thesis — *what empiricism is, and what empiricism has been* — corresponds to the first aim, and set out an extensive definition of these two versions of empiricism, traced the history of them including current manifestations, and demonstrated the neglect of ME in current philosophy of science. The second part of the thesis — *what empiricism should be* — corresponds to the second aim. Here, normative criteria were given for an empiricism suitable to current science which were derived directly from the characteristics that I gave of ME. This position was then applied to some of the most interesting research topics in current philosophy of science, all of which seem to *prima facie* move away from an empiricist line of thinking.

Chapter 1 sets out what empiricism is, and sets out the distinction between EE and ME via a set of criteria. It traces out of a history of both of these versions of empiricism, starting from the Ancient times and ending at the end of the Early Modern period with Hume. Chapter 2 continues the history of empiricism that was begun in chapter 1. It begins with various different 19th century empiricists and continues through to Reichenbach and Hempel. The aim of the second chapter is to demonstrate the fact that there has existed an alternative version of empiricism to the empiricism that is typically used by current philosophers of science. Tracing the history of empiricism has done just this. The fact that those in the ME tradition in the period of history documented are not typically seen as or listed as empiricists, despite holding to clearly empiricist doctrines, serves to illuminate this bias against empiricism that does not neatly fit into the orthodox narrative of empiricism, and does not correspond to EE.

Chapter 3 presents the current most prominent manifestations of each version of empiricism — EE and ME. This takes the form of the constructive empiricism coupled with voluntarism of van Fraassen, and the scientific empiricism of Cartwright. It also extensively highlighted the neglect of ME within current philosophy of science. To do this, reflective instances within current philosophy of science that are symptomatic of this neglect were pointed to. This came in the form of (i) van Fraassen's account of empiricism more broadly, which is supposed to account for all empiricism but fails to account for ME; (ii) Clarke's analysis of Cartwright's empiricism, which analyses it in terms of EE and fails to recognise that there is any alternative to EE, and thus gives an insufficient analysis of Cartwright's empiricism; (iii) selected quotations from prominent philosophers of science where they equate empiricism as a whole with EE, thus neglecting ME from their considerations.

Chapter 4 continues the focus of ME, this time shedding light on what ME would look like with respect to current science by deriving four normative criteria from the characteristics that were presented in (1.1). It was also argued that each of these criteria are desirable in and of themselves, and that they form the most suitable empiricism for current science. There is thus both an explorative dimension and a more prescriptive dimension to this chapter. The explorative dimension is seeing where we arrive if we apply ME to current science, and exploring the consequences of this. The prescriptive dimension argues that this is the best way to formulate empiricism. In order of appearance, the four criteria were that (i) empiricism should prioritise the collective over the individual; that (ii) empiricism should prioritise practice over theory; that (iii) empiricism should epistemically privilege measurement, and not observation; and (iv) that empiricism should embrace some sort of realism about causality.

Chapter 5 considers three fields of research within recent philosophy of science that seem, *prima facie*, to be a threat to ME, and to empiricism more generally. These are analogue confirmation, computer simulations, and NETC, and all appear to involve non-empirical elements in significant ways. I argue that the non-empirical component is not as radical as it seems for all of them, and that all do have some sort of empirical foundation. With computer simulations and analogue confirmation, I argue also that there is no irresolvable tension between both of them and the criteria that was given in chapter 4 for ME, and that they thus do not pose a threat to ME. This is not the same, though, for NETC. Here, there is a tension which cannot be resolved insofar as NETC does not and cannot plausibly epistemically privilege what is measurable.

Despite this thesis primarily being about ME, it is hoped also that much light has been shed on EE; not only by explicating EE via expicit characteristics and drawing out the history of it, but also via continuously contrasting it with ME. EE has typically been taken for granted, in recent philosophy of science, as being empiricism in its totality. It is hoped that even if the reader is not convinced of the suitability of ME as a position in philosophy of science, then the reader will at least be convinced that empiricism is not entirely tied to EE. The same is true more generally of claims made within this thesis — that even if the reader does not accept the thesis as a whole, that there are many smaller claims made that the reader will be persuaded by or at least begin to think differently about. Some of these include claims such as the need for a more practice-led approach to science within philosophy of science, a need to focus on the collective rather than the individual, that empiricism generally is not suited to the theory-led reasoning that many in the EE tradition proceed with.

Although intended to be developed as a position within philosophy of science, ME can also be used as a tool for science. This has been briefly done in (5.4.3). Using the criteria that have been put forward in chapter 4 can potentially help choose between prospective research programmes in science. This makes sense when we realise that ME has in many ocassions been a position that has been adopted by scientists who were very conscious about developing scientific methodology; ME has historically often doubled-up as both an empiricist position and a normative position on the methodology of science. We see this most clearly in Francis Bacon (1.4.1), Whewell, (2.1.2), Jones (2.1.2), Herschel, (2.1.2), Thomson, (2.1.2), and Cartwright (3.2). As stated in (5.4.3), we can see normative demands to keep an empirical methodology by people such as Ellis and Silk (2014), Hossenfelder (2018), Smolin (2006), Kragh (2011), and Woit (2006) as being in the spirit of ME here.

The thesis has also been constrained by the scope that it has tried to cover. In future work, a more extensive historical account of ME and its development would be a worthwhile project that could provide, amongst other things, more explanation on why ME came to be neglected and EE came to dominate. Future work on this topic could similarly cover a more extensive laying-out of ME as a position in philosophy of science. The aim of chapter 4 was simply to provide criteria for ME in light of current science and is consequently not anything close to exhaustive; it would be a worthwhile project to spend time developing this position in more depth than was able to be afforded here. A similarly worthwhile project would be a more extensive account of ME in current philosophy of science. There are various philosophers of science working now who either call themselves empiricists, or whos work can be viewed as empiricist, that would be enlightening to analyse in the framework presented here. Some of those philosophers include Nora Boyd, Erik Curiel, James Ladyman, John Norton, Quentin Ruyant, Karim Thébault.

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